Journal of the History of Medicine and Allied Sciences

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Three Unpublished Letters of Edward Jenner

W. W. FRANCIS* AND LLOYD G. STEVENSON*

Letters were the chief means which Jenner used in making his discovery and his views known. Baron printed a large number of letters to and from Jenner in the Life; *many of the originals of these have not been traced, but others have been published from time to time, and more still remain unpublished in libraries. They are the best record of Jenner's many-sided interests and world-wide contacts, his generosity, his scientific enthusiasm and sense of mission, and his occasional melancholy; though his ponderous playfulness and pseudo-poetical style make them unattractive to a modern reader, and quite unlike the forthright letters which John Hunter wrote to him.*

Autographs are not uncommon, but as they have been eagerly collected Munaret's words of 1860 are still true: "Les autographes de Jenner pèsent de l'or."

W. R. LeFanu

THE defects of Edward Jenner's epistolary style pointed out by his bio-bibliographer are occasional defects. They mar certain letters, or parts of letters, but there are others in which the most sensitive of modern readers could find little to cavil at. Style apart, some of them are of great biographical and even scientific interest, and it is to be hoped that selections from the masses of his correspondence still interred in libraries may yet see the light of print.

In 1947 Dr. G. E. Hall, then Dean of the Faculty of Medicine of the University of Western Ontario, obtained an eight-page letter in Jenner's autograph for the Medical Library. This letter was purchased from Mrs. Louis H. Winslow, of Milbrook, Ontario, partly through the good offices of Dr. Leonard C. Jenner of Kingsville, a collateral descendant of Edward Jenner. In the same year another unpublished A.L.S. turned up in the Osler Library at McGill University, among some miscellaneous papers of no particular interest, where it was apparently overlooked by Sir William in binding up his Jenneriana. A third letter (Bibl. Osler, no. 1268, ff. 11-12) was presented to Osler in his Baltimore days by Dr. Edward Jenner Wood, of Wilmington, North Carolina.

I

Miss Newcome, recipient of the letter now preserved at the University of Western Ontario, was the elder of two sisters residing

* Osler Library, McGill University, Montreal, Canada.

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at Gresford, in the county of Denbigh, not far from Chester.[1] Although the letter is addressed to "Miss Newcome," the salutation is "Dear Ladies," and local tradition seems to have kept a livelier memory of the younger sister, Miss Maria Newcome. The parish registers at Gresford record that she was born 14 July 1780, baptised 7 September 1780, and died 29 March 1848. Her father, Henry Newcome, was Vicar of Gresford for forty years and died in 1803; her mother, Elizabeth, his second wife, died in 1783. During their father's lifetime the Misses Newcome lived in the Vicarage; after his death they moved to Gresford Parsonage, or the "Old Parsonage," described as "a sort of dower house," at the other end of the village.

> Dear old ladies they both were, but Miss Maria was also a poetess, and as clever as she was good. Her tall upright figure was frequently to be seen visiting the sick in the remotest parts of the parish, performing various acts of charity and kindness. Every Lent the two sisters used to vaccinate all the babies brought to them. There was no doctor in those days nearer than Wrexham, and no railway, so many a poor mother was spared a long trudge by their cleverness and kindness.[2]

This tribute, undoubtedly true to local feeling, perhaps suggests only casual and lady-like, if devoted, charity. Jenner's letter, on the other hand, shows that the sisters were systematic in the extreme, and that they were genuinely interested in the principles and technique of vaccination. They seem to have visited Cheltenham to learn the correct procedure from Jenner himself. The isolation of country villages and the shortage of qualified doctors at this time, and for long afterwards, justified and even necessitated some measure of lay intermeddling in the practice of medicine; the Rev. Sidney Smith, in a similar situation, was a busy amateur of medical practice.

The "Ringwood Cases" to which Jenner refers were some supposed failures of vaccination reported to have taken place at Ringwood in Hampshire. Baron gives a full account of the matter in the *Life*.[3] William Blair's pamphlet on this subject had appeared about the time of Jenner's letter, being dated 28-31 March 1808; it was entitled *Hints for the Consideration of Parliament in a Letter to Dr. Jenner on the Supposed Failures of Vaccination at*

[1] Information about the Misses Newcome of Gresford was very kindly procured for us, with great care and at the sacrifice of considerable time, by Miss S. Bailhache, City Archivist, Chester, to whom we are much indebted.

[2] This appears to be an excerpt from an article published in 1874 in the parish magazine and transcribed in the "Vicar's Book" at Gresford.

[3] Baron, John. *The life of Edward Jenner.* London, 1827-38, II, pp. 108 ff.

Ringwood.[4] The other pamphlet recommended by Jenner for the Newcome vaccine library is not as easy to trace; its author, the ingenious Dr. Scully of Totnes, has so far escaped our toils. Mr. Ring, who appears in the story of the "little Urchin" at the end of the letter, was undoubtedly John Ring (1752-1821), a London surgeon who was one of Jenner's doughtiest adherents, and who devoted the greater part of his professional life for more than twenty years to the cause of vaccination. He led a deputation into Hampshire in 1808 to investigate the Ringwood cases. Jenner spoke of him as "honest John Ring."[5]

Letter of Edward Jenner to the Misses Newcome

Dear Ladies

To you who know so well how variously my time is occupied, I scarcely need make any apology for so long delaying to answer your obliging Letter. Since your departure I have been necessitated to pay a visit to the Metropolis & stay near a fortnight there, a circumstance that has added not a little to the general confusion of my epistolary concerns; however without saying more on these subjects I will at once congratulate you on your having opend the Vaccine Campaign so successfully. May this species of warfare never terminate till you have driven from your Country the most formidable Foe that ever invaded it.

I should have enjoyed being of your party when your first visits commenc'd among the Cottagers, but must own that nothing would have delighted me more than hearing your Lectures on Vaccination delivered to the Clergyman, whom you delegated to take charge of the business in your absence.

I will now proceed to answer your questions in the order they occur.

1st Is the Mark similar
In the natural Cowpock [?]
—as to the mark—Yes; if the Pock by chance (for this is a very rare occurrence) assumed the same form as that produced by a Lancet. Observe —every case of Cowpox whether casually from the Cow or artificially excited, must *necessarily* be a case of inoculation, as the disease cannot arise spontaneously on the human Being nor by any other means than bringing the infecting Matter in contact with the skin.

2d Is the security obtained on the 8th day?—As far as I have seen always; provided the Pock has proceeded regularly up to that period of time.

3d Is it a preventive of *Itself*?
Not uniformly so.

4 Why is the Scab the least eligible Source of Infection[?] The Scab must of course be made up of Cowpock Matter form'd at different periods

[4] LeFanu, W. R. *A bio-bibliography of Edward Jenner, 1749-1823.* London, 1951, p. 154. Index-Catalogue, 2nd series.

[5] *D.N.B.* Cf. also Baron, *op. cit., passim,* and Dorothy George, *London life in the eighteenth century.* London, 1925, p. 52, citing Pictet. *Voyage de trois mois en Angleterre . . . en 1801.* Geneva, 1802, p. 333.

of its progress; hence I conclude that its edges are less likely to produce the perfect Pock than its centre; because tho the latter contains the perfect matter in its inspissated state, the former can only be that which was formed after the *areola* (the red blush that surrounds the Pustule) was complete.

The little Schoolmaster deceived me, having never appeared at Head [quarters] since he made, in your presence, his *faithful* promises.

I admire the mode in which you keep your Cases, & I think your accuracy exceeds that of any Institution or private Vaccinator in the British Empire, perhaps in the World.—Your failing so frequently to infect must have arisen from the Lancet being placed in timid hands; & to this cause I also attribute the Pustule being smaller on the 8th day that [*sic*] you usually saw it here. Dont be afraid in future to make your Punctures larger. Some of the irregularities you met with, were clearly owing to coincident diseases of the Skin. Further Comments or Observations would be needless to *you* who are so competent to make them yourselves.

The Ringwood Cases, foolishly enough, seem to have made an impression everywhere. I must request you to procure Mr Blairs Pamphlet on this subject; and you will be much pleased with the perusal of a small Pamphlet from the Pen of the ingenious Dr Scully of Totnes on Vaccination. As you will find yourselves occasionally drawn into Controversy, a Vaccine Library will be a Tower of defence against *our* Enemies.

When you write to Mrs Fryer, pray thank her for her obliging Letter, & tell her how exceedingly Mrs Jenner & myself rejoice at the happy change that has taken place in your Sisters health, & how ardently we hope for her perfect restoration. It is my intention to answer Mrs Fryer's Letter, but being full six months behind hand in my correspondence it is impossible for me to say when. After this you must perceive I write to you almost *by return of Post.*

Do I recollect Miss Pate & the melancholy interview I had with her many years since at Newport? Most certainly I do. The sad scene is as vividly painted on my mind as if it had happened but yesterday. I beg you will present my Comps. to her. She does not I dare say to this moment know that I religiously fulfill'd my promise in riding down to Bristol Wells to see the young Lady she had the charge of; but alas! I found she had been convey'd a few hours before my arrival to the Tomb! [I never] shall forget my dismal ride home.

One question of some importance still remains unanswered. A fine little Boy about six years old came to my House in Town last Spring and presented me with a Nosegay—He was accompanied by his Mother. "Pray young Gentleman who are you [?]" said I. "Edward Jenner"* said the Boy. The mother then told me that his unhappy Father died of the Smallpox just at the time her son was born, and in the same room; & that Mr Ring, who attended her, vaccinated her infant at *half an hour old.* Thus was he shielded from the horrid Pestilence that had destroyed his Father. Dont you think the little Urchins Bouquet smelt sweet? On an emergency then, no period of human existence is too early for Vaccination; but I must remark that when it is left to our choice, it may be prudent to defer the operation till an infant is a month old.

Your seal bears a *very* correct representation of the Vaccine Pustule.

* The name he rec[d] at the baptismal Font as soon as his security was ascertaind. [Jenner's note.]

With mine & Mrs Jenner's best wishes, believe me very faithfully Yours, dear Ladies,

[signed] Edw: Jenner

Cheltenham 11 April 1808

[The address reads] Miss Newcome / Gresford Parsonage / near / Chester / double only [i.e., two sheets].

The urchin's bouquet reminds one of us of the experience of an Osler Society graduate in South America when he was consulted by an intelligent native Indian with the curious surname Bobb. "Then what's your Christian name?" "William Osler." "How the deuce??!" It seems that the village midwife was busy, and his mother was so proud of being delivered by a British Army surgeon that she wanted to give the baby his name. The wily old boy apparently dodged the honor of an homonymous piccaninny by persuading her to call the baby after the recently deceased "greatest doctor who ever lived." How Osler would have laughed!

II

The second letter, discovered in the Osler Library in 1947, was written to Dr. (afterwards Sir) Richard Dobson, of the hospital ship *Trusty* in 1814. Dobson had entered the navy in 1795 and had been surgeon of the *Lively,* when this frigate, in conjunction with three others, captured four Spanish galleons, with immense treasure on board, off Cadiz. His later services on board the *Trusty,* which he joined in 1809, were so highly valued by the Emperor of Russia and the King of Denmark, some of whose nationals were under his care, that a diamond ring and a knightly order were conferred on him by each of these monarchs. In 1814 he was appointed surgeon to the Royal Marine Infirmary at Chatham, in 1824 surgeon to Greenwich Hospital. In the latter year he was created doctor of medicine (St. Andrews). He was knighted by William IV in 1831 and retired on full pay in 1844, after a service of forty-nine years. He died in 1847.[6]

The Captain Milman who is mentioned at the beginning of the letter was one of the three sons of Sir Francis Milman (1746-1821), who had been physician in ordinary to the king since 1806 and had been President of the Royal College of Physicians in 1811 and 1812. Another son, Henry Hart Milman (1791-1868), was to become Dean of St. Paul's and a famous ecclesiastical historian.[7]

[6] Munk, William. *The Roll of the Royal College of Physicians of London.* London, 1878. III, pp. 299-300.

[7] Sir Francis Milman and Henry Hart Milman are both in the *D.N.B.*

Captain Husson, the French officer who seems to have been traded for Captain Milman in an exchange of prisoners, was a brother of the eminent physician, Henri-Marie Husson (1772-1853), one of the earliest and most active of the proponents of vaccination in France.[8] Dr. Husson had vaccinated Napoleon's son in 1811 and his good offices, together with those of Corvisart, were sometimes employed by Jenner in procuring the release of British prisoners. To the great indignation of John Baron, Husson published in 1821 an article in the *Dictionnaire des Sciences Médicales* which advanced the (rather thin) claim of M. Rabaut, a Protestant clergyman of Montpelier, to be the discoverer of vaccination.[9]

Captain Husson had had a bad time of it.[10] On 22 July 1808 the French general Dupont surrendered his army to the Spaniards at the Capitulation of Baylen. According to the arrangement made on the spot, the French forces were to be allowed to leave Spain. The Junta of Seville, however, refused to ratify the terms of the capitulation. All the French except the superior officers were sent to the galleys at Cadiz. Some of the officers went to England as prisoners of war and one of these was Captain Husson.

"I petitioned for his release," wrote Jenner. "It was the first request of the kind I had made to the British Government; and it seemed to meet with a favourable reception. This joyful intelligence I communicated to Captain Husson: when, most unexpectedly, instead of acquiescence, a refusal arrived. This threw him into a state of desperation, and in the midst of it he broke his parole, and is now in a state of misery and confinement on board a prison-ship at Chatham. This hurts me excessively: as I cannot but look on myself as in some measure the innocent cause. I shall again exert myself for Captain Husson, conceiving that my first application could not have been well understood."

Baron thought that the second application had been successful, for Jenner had written to him, 5 July 1811, "This day I received a letter from town, informing me that my petition to the Prince in behalf of Captain Husson had been graciously received." But something must have gone wrong, for, as the following letter reveals, the unfortunate Captain was still a prisoner, and was probably still in confinement at Chatham, at the beginning of 1814. At this time, apparently, Jenner at last effected his release. Baron is

[8] *Dictionaire des Sciences Médicales. Biographie Médicale.* Paris, 1820-25, t. 5, pp. 321-322.

[9] Baron, *op. cit.*, I, pp. 548 ff.

[10] What follows is based on Baron, *op. cit.*, II, pp. 164 ff.

inclined to think that Dr. Husson's "altered manner . . . in speaking both of Dr. Jenner and vaccination" may have been due to misunderstanding of Jenner's rôle in the matter of his brother's long imprisonment. He may have "believed Dr. Jenner alike insensible to the calls of gratitude and of duty," as well as negligent about answering letters. The opening paragraph of the newly discovered letter printed below helps to confirm Baron's claim that "Jenner felt for young Husson as for a son; and exerted himself with unremitting earnestness to procure his liberation."

Jenner's method, not peculiar to himself, of guarding against contagion was somewhat heroic. "The "oxymuriatic gas" employed was chlorine, discovered by Scheele in 1774; but whereas Scheele treated *Brun-sten* (braunstein, magnesia nigra, the native dioxide of manganese) with spiritus salis (hydrochloric acid) to produce chlorine by a method still in use,[11] Jenner's recipe called for manganese (undoubtedly the native dioxide), common salt, and sulphuric acid: $MnO_2 + 2\ NaCl + 2\ H_2SO_4 \rightarrow MnSO_4 + 2\ H_2O + Cl_2 + Na_2SO_4$.

One mystery is unsolved. What divagations of the Tsar, or of the British government, are referred to in the paragraph on Russian Honours remains for the present (at least for the present editors) an enigma. The honours are presumably those given to (or temporarily withheld from?) Dr. Dobson himself, but what enormities so angered Jenner's medical friends we cannot guess. There seems reason to suspect a touch, if no more, of Jennerian hyperbole. Not long afterwards Jenner himself was received in London by the Emperor of Russia, and although he began the interview by contradicting majesty, he ended it, according to his own account, very nearly tongue-tied with awe.[12]

Letter of Edward Jenner to Doctor Dobson

My dear Sir

You little thought at the time you were writing your Letter of the 23d Jany: so full of despondency with regard to poor Capt. Hussons liberation, how soon that long wished for event was to take place. The Gallic Emperor has yielded to my solicitation for the release of Capt. Milman (son of Sr F. M.) and thus I obtain the Key of the Prison House which holds Captain Husson. I withold the history which led to the accomplishment of my wishes, conceiving it would be a twice told tale. But where is the Captain—is he off? I am impatient to hear of him & know you will indulge me with some information as soon as convenient. If he should be permitted to take

11 Cf. Chattaway, F. D. The discovery of chlorine by Scheele. *Chem. News*, 1910, *101*, p. 25.

12 Baron, *op. cit.*, II, p. 207.

a ramble before he sails, pray tell him how happy I should be to see him here, & at the same time present him with my most hearty congratulations on the termination of his captivity. Until the business was finally settled, I was unwilling to acquaint him with the foundation of my hopes, strong as it was; having once wound up his feelings to a pitch of extacy, & then plunged them, from sad necessity, into despair.

Contagion—I tremble at the very word, having no less than three times in my life been nearly its victim from attending typhoid Bed sides. I sincerely hope you may escape its fangs. For some years past, tho' often expos'd (what a horrid profession is ours!) I have wrapp'd myself in the oxymuriatic gas, & to this I attribute my being uninjured. I do this by holding in my hand a short wide mouth'd vial with Salt & Manganese, moisten'd with sulphuric acid. Excuse this hint.

I have taken the liberty of shewing your communication relative to the Russian Honours to a few of my medical Friends. So indignant did they feel as well as myself, that I must not exhibit it again; desertion may be the consequence. But in what point of view is the Emperor plac'd by this transaction?

You speak of the Vaccine—I will just ask if you know how it happens that the Navy is not plac'd under its protecting power as well as the Army? I often hear of its making great havoc among our Ships at Sea. Before a Ship sails the men should be inspected by the medical Officers.

Believe me, dear Sir, with great respect
very faithfully your's
[signed] Edwd Jenner

Cheltenham / Feb: 16th 1814

[Address, with postmark 18 February:] To / Doctor Dobson / Hospital Ship Trusty / Chatham

III

The third letter (Bibl. Osler, 1268, ff. 11-12) is undated and the postmark is so smeared as to be illegible except for the word, "Berkeley," but from internal evidence it seems probable that it was written in 1816. It is addressed to Jenner's son, Robert Fitzharding Jenner, who outlived his father by many years but died without children. (Jenner's other son, Edward, died in boyhood, so that there are no direct descendants in the Jenner line from Edward Jenner himself.) Young Robert had been vaccinated in infancy by his father; the vaccination had failed to "take," and when he was later exposed to smallpox, Jenner, having no vaccine at hand, became very much alarmed and as a second best inoculated the child with smallpox virus, reverting to the old method of variolation. Enemies of vaccination, getting wind of this occurrence, charged that the author of the new method did not believe in it himself.[13]

[13] *Ibid., II,* pp. 43 ff.

The second paragraph of the letter below gives a clue to its date, since it evidently has reference to a local tragedy described by Baron. In January 1816

> A desperate conflict had taken place between a gang of poachers and the gamekeepers of Lord Segrave, in which one of the latter lost his life. Dr. Jenner, as a magistrate, was obliged to exert himself on this occasion, and he assisted materially in procuring and arranging that chain of evidence by which the guilt was most clearly brought home to the murderers; two of whom suffered the extreme penalty of the law, and their companions, twelve, I believe, in number, were transported for life. . . .
>
> Dr. Jenner attended the trial at Gloucester. . . . He was very much affected by the result of it. Most of the culprits were young men, and the sons of respectable farmers; and though he laboured for the punishment of the guilty, he could not but lament the consequences of the tragedy, which carried such lamentation and woe into so many families in his neighbourhood.[14]

With the exception of the Ducies, the Kingscotes, and the Davies family, none of the persons whose names appear incidentally in this letter have been identified. Lady Frances Moreton, afterwards Lady Ducie, is mentioned by Baron as "the first personage of rank who broke through the chains of prejudice, and had her only child vaccinated";[15] the Kingscotes are briefly referred to by Baron as among Jenner's friends; the Rev. William Davies was his brother-in-law.

Letter of Edward Jenner to his son, Robert Jenner

My dear Robert,

You are gone poaching then, are you, to Mr Bolton's? Where is your Certificate? Tell Mrs Kingscote she shall hear from me, & tell her too that the persuasive eloquence of Women is something like music. It can "soften Rocks & bend the knotted oak"

The *Detenus* at the Castle increase—We have a brace more there on suspicion. I suppose it is known at Kingscote that one of Lord Ducies Keepers has sworn to Allen Vickery & a party went down to Bristol Tuesday night, as we had scent of some of the Villains lurking there; but they are not yet returned. Murder will out, depend upon it.

I was extremely vext at finding you had often thrown out hints of not keeping the next Oxford Term. In every point of view this would have been injurious to you. Of course I am much pleasd at what you mention on this subject in your Note. My satirical Song I find was quite thrown away upon you.

"How pleasant to meet
In a shower of sleet"
&c &c &c

I heard of your set off to the Hunt!!

Yr affectionate Father
[signed] Edwd Jenner

[14] *Ibid.*, II, pp. 221-22.
[15] *Ibid.*, I, p. 304.

The Davie's from Stanley come Saturday. Take care of Harriet.
[Address:]

DURSLEY / Penny Post BERKELEY [date illegible]
R. Jenner Esq[r]. / Kingscote / Dursley
To be opened if / not there

These three letters, written over a period of about eight years, show their author in various humours and capacities—writing in a gallant, and at the same time avuncular, style to two country ladies who are carrying forward his crusade; writing with fewer flourishes to a professional colleague in a letter which yet breathes forth friendship, loyalty, and anxiety; and writing at midnight to his son, much relieved by the bettered resolution of the unwilling scholar, the recent cause of a father's extreme annoyance which had vented itself—how else but in a satirical song?

John Hunter and the Unfortunate Doctor Dodd

JESSIE DOBSON*

ON Friday, 27 June 1777 the execution took place at Tyburn of William Dodd, Doctor of Laws, former Prebendary of Brecon and Chaplain-in-Ordinary to the King. The events that led to this catastrophe were as follows.

William Dodd, son of the vicar of Bourne in Lincolnshire, was born on 29 May 1729. He entered Clare Hall, Cambridge, was said to be diligent at his studies, and took his degree with distinction in 1749, his name standing fifteenth on the 'First Tripos List.' Instead of seeking employment, he went to London and seems to have led a gay life, full of pleasures and parties, gaining some little notoriety for his verses and other writing. It was at this period that he became acquainted with Mary Perkins of Frith Street, Soho, a lady "largely endowed with personal attractions but deficient in those of birth and fortune." So well did this friendship thrive that they were married on 15 April 1751 and began their wedded life in a house in Wardour Street. It was while he was living there that he published an elegy on the death of Frederick, Prince of Wales. His father, however, who had always desired that his son should enter the church, was greatly distressed on hearing of the kind of life he was leading and now insisted that he should return to Cambridge. On 19 October 1753 William Dodd was ordained a priest, the Wardour Street house was given up, and he was appointed curate to the Rev. Mr. Wyatt at West Ham. Here he discharged his duties with zeal and became known as a good preacher. He gave public lectures, wrote a novel entitled *The Sisters*, and, to augment his income, took a few young gentlemen as pupils. When the Magdalen House in St. George's Road, West Ham, was opened in 1758, he preached the inaugural sermon and later acted as chaplain, receiving a salary of £100 per annum. The charity was popular and was patronized by many distinguished people who came to hear Mr. Dodd's sermons which were said to be so moving that the 'lost sheep'—and even some of the patronesses—wept. He now began to publish a series of edifying books and was a regular writer for, and later editor of, the *Christian Magazine*.

* Royal College of Surgeons, London, England.

In 1763 he was appointed Chaplain to the King and also to Bishop Samuel Squire of St. David's, who gave him a Prebend at Brecon. In 1766 he gained his LL.D. degree and in 1770 published his *Commentary on the Bible* that had previously been issued in monthly parts. It was through Bishop Squire that he was appointed Tutor to Philip Stanhope, nephew to Lord Chesterfield. He now resigned his curacy at West Ham, took a house in Southampton Row, and bought a country residence at Ealing where he received as pupils the sons of good families. At about this time his wife received a legacy of £1500 and won a prize in a lottery to the value of £1000. With this money, Dr. Dodd built a chapel in Pimlico that he named Charlotte Chapel after the Queen. Here he attracted large and fashionable congregations and gained much social prestige. This, however, seems to have undermined his moral character, for his name began to be associated with various scandals and he found that his income was not adequate to meet the many demands made on it by his more luxurious style of living. In 1772 he was preferred to the Rectory at Hockliffe in Bedfordshire, but this did not satisfy his ambitions and after two years it is said that he or his wife made an attempt through influential friends, one of whom was Lady Apsley, to gain appointment to the living of St. George's, Hanover Square. For this injudicious behaviour, Dr. Dodd was struck off the list of Chaplains and he found it prudent to live abroad for a time, where he visited his former pupil, now Lord Chesterfield, through whose good offices he was soon afterwards presented with the living of Wing in Buckinghamshire. This brought his income from preferments up to £800 a year but it was not long before he began to get into financial difficulties again and in 1776 he disposed of Charlotte Chapel. The proceeds of the sale, however, did not settle all his debts and, being in urgent need of some £300 to pay tradesmen's bills, he approached Mr. Robertson, a broker, to arrange for a loan. The story he told was that Lord Chesterfield was desirous of borrowing a few thousand pounds but, not choosing to make the matter public, wished the affair to be conducted with the utmost secrecy and Dr. Dodd had undertaken to negotiate this for him. He produced a counterfeit document, purporting to be the bond of Lord Chesterfield and supposedly signed by him. Mr. Robertson added his name to the bond, apparently acting in good faith, and the money was advanced by Henry Fletcher and Samuel Peach.

All might have gone well for Dr. Dodd had not Mr. Manly, attorney for Mr. Fletcher, noticed a blot or blemish on the original document and thought it advisable for a new bond to be made out. This was done and Mr. Manly took it for signature to Lord Chesterfield who denied all knowledge of the transaction. Mr. Manly and Mr. Fletcher thereupon went to Guildhall to lodge information against Dr. Dodd and Mr. Robertson respecting the forgery. Dr. Dodd was called upon to make restitution, which he at once did, in full, but a charge was made against him before the Lord Mayor. On 19 February 1777, Dr. Dodd appeared at the bar of the Old Bailey, the facts were proved, and he was found guilty. To the question as to why he had forged a bond amounting to £4200 when his debts amounted to only a few hundred pounds, he replied that no one would believe that Lord Chesterfield would wish to borrow £300 in secret but that it would be quite feasible that he would be unwilling for it to be known that he was embarrassed for so large a sum. Dr. Dodd made an excellent speech in his own defence and pointed out that no one had suffered and that the money had been restored. The prosecution, however, suggested that surely Lord Chesterfield himself, if approached, would have obliged his old tutor with the loan of a few hundred pounds or, alternatively, that Dr. Dodd could have raised the money himself on the security of his goods or preferments. Furthermore, it was disclosed that he had actually entered into an arrangement with some French artists to produce a new and splendidly engraved edition of Shakespeare and it was assumed that he needed money for this project. On the first day of the sessions held at the Old Bailey in May 1777, Dr. Dodd appeared before Mr. Justice Aston and on May 16th, the last day of the sessions, was sentenced by him to be hanged.

It has been stated that Dr. Dodd had many influential friends and among these was the Countess Harrington, daughter of the Duke of Grafton, wife of William Stanhope, second Earl of Harrington, himself a distant relative of Lord Chesterfield. This lady was one of the reigning beauties of the times and was a close friend of Horace Walpole. She now wrote on behalf of Dr. Dodd to Dr. Samuel Johnson who had met the prisoner on one occasion many years previously. Boswell gives the account of how Johnson reacted towards her request. "Mr. Allen, the printer, who was Johnson's landlord and next neighbour in Bolt-Court, for whom he had much kindness, was one of Dodd's friends, of whom, to the credit

of humanity be it recorded that he had many who did not desert him, even after his infringement of the law had reduced him to the state of a man under sentence of death. Mr. Allen told me that he carried Lady Harrington's letter to Johnson, that Johnson read it, walking up and down his chamber, and seemed much agitated, after which he said 'I will do what I can' and certainly he did make extraordinary exertions." It is a fact that Johnson wrote much of Dr. Dodd's speech to the Recorder of London at the Old Bailey when sentence of death was about to be pronounced upon him. He wrote also 'The Convict's Address to his unhappy Brethren,' a sermon delivered by Dr. Dodd in the Chapel of Newgate, and composed two letters, one to the Lord Chancellor Bathurst and one to Lord Mansfield, as well as Dodd's speech on the scaffold. On Sunday, June 22d, five days before the date fixed for his execution, Dr. Dodd wrote to Dr. Johnson begging his assistance once more in framing a letter to the King. He says: "If His Majesty could be moved of his royal clemency to spare me and my family the horrours and ignominy of a publick death, which the publick itself is solicitous to wave, and to grant me in some silent distant corner of the globe to pass the remainder of my days in penitence and prayer, I would bless his clemency and be humbled." Dr. Johnson acceded to the request and composed a letter which he sent to Dr. Dodd with the following covering note: "Sir, I most seriously enjoin you not to let it be at all known that I have written this letter, and to return the copy to Mr. Allen in a cover to me. I hope I need not tell you that I wish it success—But do not indulge hope. Tell nobody."

In spite of all the petitions, one of which was supported by twenty-three thousand signatures, the appeal for exercise of the Royal mercy failed and, two days before his execution, Dr. Dodd wrote again to Dr. Johnson as follows: "Accept, thou great and good heart, my earnest and fervent thanks and prayers for all thy benevolent and kind efforts in my behalf. Oh, Dr. Johnson! as I sought your knowledge at an early hour in life, would to heaven I had cultivated the love and acquaintance of so excellent a man! I pray God most sincerely to bless you with the highest transports —the infelt satisfaction of humane and benevolent exertions! And admitted, as I trust I shall be, to the realms of bliss before you, I shall hail your arrival there with transports, and rejoice to acknowledge that you was my Comforter, my Advocate and my Friend. God be ever with you!"

The unfortunate Dr. William Dodd
A contemporary print. Reproduced from *A Famous Forgery*
by Percy Fitzgerald, M.A., F.S.A. London, 1865.

It is said that on the day before his execution, the public were admitted to see the prisoner on payment of a shilling fee. On Friday, June 27th, he was taken to Tyburn which was not far from the place where Marble Arch now stands, accompanied by the Rev. Mr. Villette and the Rev. Mr. Dobey. According to the account that appeared in the *Gentleman's Magazine* (vol. 47, p. 92): "On this occasion there was perhaps the greatest concourse of people ever drawn together by a like spectacle. From Newgate to the place of execution the streets were thronged and never were seen so many weeping eyes." Dr. Dodd then delivered his final speech, ending with the words: "I confess with deepest compunction the crime which has brought me to this place and admit the justice of my sentence, while I am sinking under its severity."

After hanging for about an hour, the body was put into a carriage and taken to the house of Mr. Davies, an undertaker, in Goodge Street, where medical men were waiting to make the experiment of endeavouring to revive him. It is at this point that the story becomes confused but the following account gives all the information that appears to be available.

In September 1777 was published in the *London Review of English and Foreign Literature* (vol. 7) an imaginary 'Dialogue in the Shades' supposed to have taken place between Dr. Dodd and a recently deceased Member of Parliament. On page 233 of the same volume, a correspondent makes the following observation: "An Irishman of our acquaintance assures us very seriously that this dialogue could not have happened, as he is ready to take oath that, to the best of his knowledge, he dined with the Doctor, after he was dead, at Dunkirk. It is true, he owns, he had before no personal knowledge of him; but that he was well known by those who told him it was the unfortunate divine." Apparently this item of news was not taken seriously and it was not until 1790 that the affairs of Dr. Dodd were brought to notice again by a letter printed in the *Gentleman's Magazine* (vol. 60, p. 1010) asking for information about his interment. This brought numerous replies, one of which states that the "person in question was buried with lime in his coffin, at Cowley in Middlesex, on the north side of the church and close to it, towards the east, where not a stone tells where he lies."

This announcement, seemingly authentic, should have disposed of the matter, but this was not the case. In the *Aberdeen Journal* issued on Tuesday, 19 August 1794, appeared this report:

The following account of the Resuscitation of the late Rev. Dr. Dodd is contained in the copy of a letter found in the repositories of a Gentleman of Glasgow, lately deceased:

Sir,

Now that Dr. Dodd is beyond the reach of his enemies, you may acquaint them he is here, in sound health, though in melancholy spirits; depressed in his mind at the idea of quitting for ever his native country; and being necessarily compelled to hide his head from public conversation, which was in England his chief enjoyment. Gifted by nature with the most shining talents of speech, it must be a great mortification to him, that all the courtship of popular applause is at an end, and that he must sink in obscurity, after raising himself to the pinnacle of admiration. He is at the house of Monsieur du Pee, who, being my relation and particular friend, I have had an opportunity of seeing the Doctor—The accounts he gives of his deliverance, which he gathered partly from the information of those to whom he is indebted for his life, and partly from his own knowledge, are these:

He says, he had always conceived that those warm advocates, and as he often calls them, unparalleled proofs of generous friendship, Mr. H. and Dr. C would attempt to restore him after the apparent execution of the law. He had no other reason for the supposition, than a surmise that arose naturally, when he considered that, in many instances, they had shown to the world the possibility of reviving the functions of life: not the most distant intimations were ever given of their design; though he confesses, that till the night before he went to Tyburn, he relied with implicit security on the clemency of his Sovereign, and even entertained some hopes the next morning, notwithstanding the King was at Kew, and no respite heard of at the Secretary's office. He says he never could summon resolution enough to reflect on the day he was to be exhibited a public spectacle, without horror, which seized him at intervals in the coach, and caused his imagination to swim into absolute insensibility. When he was turned off, he felt a sudden impulse of pain at first, but, by his body whirling round very swiftly, he was soon deprived of all sensation, and afterwards remained totally senseless, until he found himself in bed, surrounded by Doctor C, Mr H, Mr D and Mr W, whom he perceived to be in tears, which may be considered as an effusion of joy at his recovery, of which they at one time despaired. From them he learned, that they had conveyed him from the crowd with difficulty to where he was, and had previously ordered matters for the experiment they had determined to try to bring him to life. Mr H and Mr D stripped, and exercised friction on his corpse for two hours, without the least symptom of success: at last they perceived a motion of his breath, which convinced them of the practicability of the design, and this omen was followed by a sweat that spread itself over the Doctor's body, and a continual panting and groaning. The return of the blood to its proper situation gave the Doctor so much pain, that, he says, life was hardly worth purchasing at so dear a rate.

Maybe the disturbing events reported from France and the stirring naval encounters of this year caused the above account to be, apparently, entirely overlooked. It certainly aroused no comment at the time and it was not until 1822 that the affair was brought to light again. In the *Newcastle Magazine* for January of that year (*n.s.*, vol. 1, p. 18) a correspondent writing under the

pseudonym of 'Onesimus,' who had evidently been studying the reports relating to Dr. Dodd, queried whether he really had been smuggled away after the execution, as had been suggested. A reply to this letter, from Charles Hutton (1737-1823), F.R.S., LL.D., was published in March on page 127 of the same volume, as follows:

In addition to the report of the many stories concerning the case of Dr. Dodd who was executed for forgery in the year 1777, that have been very sensibly stated by your correspondent 'Onesimus' in a well written account in your first number, which contains, I believe, all the various accounts of the case that have been at different times in circulation, I have it in my power now to add what may be considered as a positive statement of the truth in that case, which is, that the best means were employed for the doctor's recovery, by a numerous collection of the medical faculty; but all without the desired effect. This, Sir, is a fact which I doubt there is now no other person in existence to testify, and the short history of the matter is as follows:

Soon after I had the honour, after an honourable competition, to be appointed to the Professorship of Mathematics in the Royal Military Academy at Woolwich, in the year 1773, I had the year after also the honour to be elected a member of the Royal Society of London. This situation brought me into immediate and intimate habits of familiar acquaintance with most of the more scientific characters of that respectable body, and about the metropolis. Of these there was a small select number of gentlemen who constituted themselves into a certain club that, after the usual weekly meeting of the Royal Society, retired to Slaughter's Coffee House at the top of St. Martin's Lane, to eat a few oysters and hold familiar discourse together on the subjects that had occurred at the meeting and on any other current scientific matters. I constantly attended this club as an adopted member, which consisted of such characters as Constantine, Lord Mulgrave, who, in the year 1773, sailed to near the North Pole to explore the northern regions; also of Mr. Banks, afterwards Sir Joseph Banks, who had sailed round the world with Captain Cook and was afterwards President of the Royal Society for more than forty years; also of his assistant and fellow traveller, Dr. Solander; also of Doctor Blagden, afterwards Sir Charles Blagden and his brother, Mr. Blagden, a lawyer; also of Dr. Geo. Fordyce and of Mr. John Hunter, the celebrated surgeon and anatomist, the brother of Dr. William Hunter; and of several other regular members and occasional scientific visiting gentlemen.

Now I well remember, that at one of these meetings of the club, in the year 1777, soon after the execution of Dr. Dodd, the conversation turned on that subject and on the various reports of the attempts to recover his animation and it was seriously requested by the company then present, that Mr. Hunter, who was known to be one of the medical gentlemen who attempted that experiment and who was, indeed, the chief operator in it, would state the result, to which request he readily and solemnly replied, that it was very true that he and several of the brethren of the faculty had associated together for the purpose of making that experiment and procured the body as soon as they possibly could, which was not so soon, however, as they wished and that they tried all the means in their power for the reanimation,

but unfortunately without the desired effect as the experiment entirely failed!

That, Sir, you may consider to be a certain fact, as Mr. Hunter was that kind of character for integrity and boldness to declare the full truth and to despise any subterfuge or concealment, especially among a company where the utmost confidence and familiarity had so long prevailed and had endeared the company to each other. And, beside this evidence, Sir, I think it quite in vain to expect any other or better, as I believe there is now no other person living who was present at that conversation; nor, indeed, any person in existence who was present at the experiment above related by Mr. Hunter.

This letter is dated 4 February 1822 and a note in the journal on page 138 states that by a curious mischance the original was lost and never found so that Mr. Hutton was asked to send another copy, this explaining why it was not published until March.

Through the kindness of the Rev. G. C. Gilson, the present Rector of St. Laurence Parish Church, Cowley, Middlesex, it has been ascertained that there is a memorial stone in the churchyard there, bearing the following inscription:

CLOSE TO THIS SPOT IS BURIED
THE REV. DR. WILLIAM DODD.
AUTHOR, AND AT ONE TIME
CHAPLAIN TO KING GEORGE III.
HE WAS HANGED AT TYBURN IN
THE YEAR 1777 FOR FORGERY.
JESU MERCY.

A little more information is available from an address entitled "Samuel Johnson and the Dodd Affair," delivered at the Annual Meeting of the Johnson Society at Lichfield on 15 February 1951 by the Rev. W. E. Foster. He states that the Rev. Weeden Butler, who for many years acted as Dr. Dodd's secretary and assistant, conveyed the body to Cowley and that the tablet mentioned above was placed in position during the incumbency of the Rev. W. R. C. Hamilton, Rector of Cowley from 1902 to 1930. Previously, the grave had been marked by a stone bearing only Dr. Dodd's name, age, and dates of birth and death. It is curious that the parish registers contain no entry for the interment. The rector in 1777 was Richard Dodd, brother of the unhappy man whose punishment, though strictly in accordance with the law of the period, may appear to have been unduly harsh. He retired in 1807, and Mr. Gilson mentions that a benefaction bequeathed by him in 1811 is still administered by the Parish.

There seems no reason to doubt that Hunter carried out this attempt at resuscitation. In his Lectures on Surgery he states that he had been studying the 'vital principle' for more than thirty years and this would be one of the dozens of experiments that he made in an attempt to elucidate the problem. He was of the opinion that "the principle called life cannot arise from the peculiar modification of matter, because the same modification exists where this principle is no more . . . Life, then, appears to be something superadded to this peculiar modification of matter." From this he proceeds to the conviction that "every individual particle of the animal matter, then, is possessed of life, and the least imaginable part which we can separate is as much alive as the whole." In his treatise *On the Blood,* he further maintains that "the blood has life." Later he speculated with the idea that as each particle contains the living principle, "some rearrangement of the particles may deprive them of it . . . the phenomenon of life may be necessarily connected with or even dependent on such an arrangement." His numerous 'freezing' experiments, designed to discover whether it was possible to revive animals subjected to temperatures lower than the minimum considered compatible with life, supported his theory of the variation in individual susceptibility. Incidentally, he was most disappointed with his results in this series for, he says, "Till this time, I had imagined that it might be possible to prolong life to any period by freezing a person in the frigid zone, as I thought all action and waste would cease until the body was thawed. I thought that if a man would give up the last ten years of his life to this kind of alternate oblivion and action, it might be prolonged to a thousand years; and by getting himself thawed every hundred years, he might learn what had happened during his frozen condition. Like other schemers, I thought I should make my fortune by it; but this experiment undeceived me." But his lack of success in this direction by no means abated his curiosity about the 'vital principle.'

In 1767 a society was formed in Amsterdam with the aim of saving those who, by accident or intention, were in danger of losing their lives by drowning. Similar institutions were established in other European cities and in Hamburg notices were read in all the churches giving directions for the treatment not only of the apparently drowned but also the strangled and those suffocated by noxious vapours. In 1773, Thomas Cogan, M.D., translated into English the Memoirs of the Amsterdam Society, so drawing the

attention of William Hawes, M.D., to this subject, and it was due to the influence of these two gentlemen that the Royal Humane Society was founded in the following year. Its activities, at first confined to the recovery of drowned persons, were later extended to include every known cause of apparent sudden death. John Hunter's name appears in the first list of directors. In 1775 the Rev. William Dodd was made a Life Governor and on March 10th in the following year he preached the Anniversary Sermon at St. Andrew's, Holborn, when £44.15.3. was collected for the benefit of the Society. He was also the preacher at St. Margaret's, Westminster, on 2 June 1776 when the contributions for the same purpose amounted to £34.3.4. When disgrace fell upon him, his good friends in the Society did not desert him. At the Annual General Court held on 5 May 1777 it was unanimously resolved to present a petition to the King on his behalf.

It was in 1776 that John Hunter was requested by William Hawes to give his ideas on the subject of the recovery of persons apparently drowned and this he did in a paper read before the Royal Society on March 21st and published in the *Philosophical Transactions* (vol. 66, p. 412). In his introductory remarks, he states that he was of the opinion that in all violent deaths, save those caused by lightning or electricity, "at first only a suspension of the actions of life has taken place" and that so long as the power of life is retained, action may be recoverable. He says that he had had no opportunities of making actual experiments upon drowned persons; nor, so far as we know, had he been able to apply his remedies to the apparent victims of violent death from other causes, and this most probably explains his participation in the experiment upon Dr. Dodd. Here he had the requisite conditions to test his theories and we can imagine his impatience and exasperation when the crowds delayed the arrival of the body—and his disappointment if, in fact, he discovered that his efforts were fruitless.

ACKNOWLEDGMENT

The author's most sincere thanks are due to William Veitch, Esq. J.P., Director of Aberdeen Journals Limited, and members of his staff, who so kindly searched their files and traced the letter quoted; also to the Secretary of the Royal Humane Society for supplying information from the records.

Moritz Traube, 1826-1894: His Contribution to Biochemistry

THEODORE L. SOURKES*

I

THE story of the nineteenth century conflict between the mechanist and the vitalist outlooks in science may very appropriately be told from the standpoint of enzyme chemistry, the field where much of the battle was fought. Fundamentally a dispute over the applicability and the relation of chemistry to life phenomena, the so-called "Liebig-Pasteur controversy" emerged as a dispute over the mechanism of fermentation and the existence of enzymes mediating the fermentation process. So sharply drawn were the lines and so influential were the rôles of the chief protagonists in the contest that other viewpoints, which cannot be characterized as in agreement with either Liebig's or Pasteur's theories, have not received the attention they merit. Among the biochemists presenting a fresh approach must be included Moritz Traube, aptly named by Müller (10), *der geistvolle Weinhändler aus Ratibor*. By comparison with his contemporaries Traube held advanced views on the relation of chemistry to physiology, on the mechanism of chemical change in living matter, and on the nature of enzymes. He is probably best known for his invention of artificial semipermeable membranes, but he should also be remembered for his studies on autoxidation and catalytic oxidation. From the remarkably wide range of research topics which he investigated in the course of his career, only a few which illustrate his contribution to biochemistry will be dealt with here. His work was bound together by a consuming interest in the rôle of oxygen in organic life; out of it came the development of his theory of enzymes and fermentations which, now almost one hundred years old, was the forerunner of the modern view (7, 13).

II

Traube was a native of Silesia.[1] He was born on 12 February 1826, in the small provincial town of Ratibor where, as the third son of a wine merchant, he had his initial schooling. He later attended the University of Berlin, interrupting his course there to

* The Allan Memorial Institute of Psychiatry, Montreal, Canada.

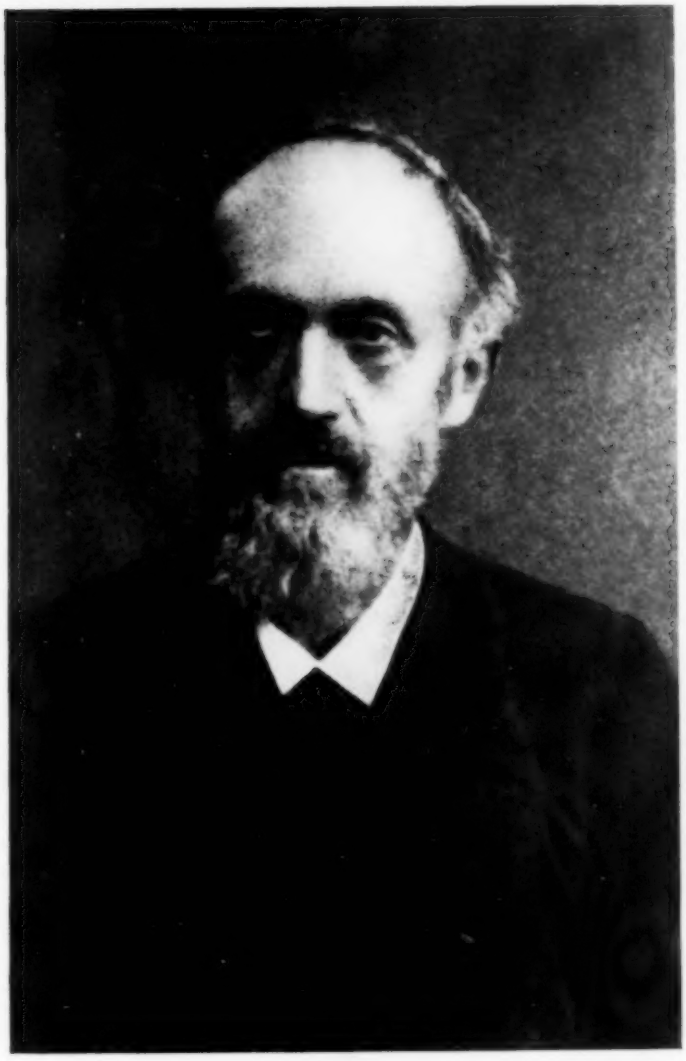
[1] Details of Traube's life may be found in an article by Bodländer (1).

study in Liebig's laboratory at Giessen under the supervision of Hofmann and Will. Returning to Berlin he studied the chemistry of chromium compounds and subsequently received his doctorate in 1847. His first position was with a dye manufacturing firm in Berlin, but he soon left it to enter the medical faculty at the University of Berlin; he was probably influenced in this decision by his brother, Ludwig, the eminent clinician. However, before Traube had progressed very far in his studies, he was called home to assist in the management of his father's business. This turn of events cut him off from direct contact not only with medicine, but also with academic science; yet it directed his attention to a chemical problem which he did much to solve.

In spite of the demands of his new career he was able to satisfy his interest in science by establishing a small basement laboratory where he could work after hours. His first publications appeared in 1852, three years after his return to Ratibor, and his next, dealing with his theory of enzyme action, six years later. Thereafter a fairly regular stream of papers issued from his "private laboratory." One is led to assume that by this time he had established a *modus vivendi,* permitting him to fulfill his responsibilities both to the family business and to his scientific aspirations.

In Ratibor, however, he missed the intellectual stimulation to be gained by contact with fellow scientists. This was instrumental in his decision to remove home, business, and laboratory in 1866 to Breslau (now Wroclaw), the capital of Silesia, where he could be closer to the scientific activity of the province centered at the University. Here were his associates, Poleck, professor of pharmaceutical chemistry and a friend from his Giessen days, and Heidenhain, the head of the Physiological Institute. In Breslau he continued his dual rôle of businessman-scientist for twenty years, when he retired from business to devote all his time to research. Among the assistants who aided him at various times in his research were Guido Bodländer, his son Hermann Traube, and Stanislaus Krawczynski. In his last years he moved to Berlin, continuing to work until his death on 28 June 1894.

Traube was a member of the German Chemical Society and was active in the affairs of the Silesian Society for National Culture. He received a number of honors during his lifetime. In 1867 the University of Halle, at its fiftieth anniversary, awarded him the honorary degree of Doctor of Medicine, and in 1886 he was elected a corresponding member of the Prussian Academy of Science.



M. Traube

Traube married in 1855, in Ratibor. Of his five children, two achieved distinction in science: Hermann, in the field of mineralogy, and Wilhelm, in chemistry.

III

About 1850, when Traube began his scientific career, physiological chemistry was developing as a discrete discipline, stemming from the application of chemical knowledge to agricultural techniques, on the one hand, and to physiological problems on the other. By this time achievements in chemistry had destroyed the technical barrier between the organic and inorganic worlds, justifying this new approach; but a conceptual barrier continued to exist in many minds even after the basis for it had been removed. The projection of the methods and aims of the chemist into the work of the biologist required, at least in the domain of theory, a reconciliation between the two hitherto separate studies, whether or not the necessity was recognized explicitly by those in the field.

Perhaps the one problem which was the touchstone of this reconciliation was that of alcoholic fermentation. A "substance," yeast, which in small amounts could convert virtually unlimited amounts of sugar into alcohol and carbon dioxide at ordinary temperatures, had engaged the attention of the alchemists, the natural philosophers of the seventeenth and eighteenth centuries, and the modern chemists from Lavoisier on. With the discovery in the 1830's that yeast is a cellular living organism and not simply a chemical compound, a new dimension was introduced into the problem. But before this dimension had been adequately described or its existence appreciated, it was to divide the nineteenth century biochemists into two opposing camps, on either side of a chasm which already existed in biological science. These were the "vitalists," holding at first to the views of T. Schwann and later of Pasteur, and the "mechanists," including Wöhler, Liebig, and many other chemists. For the former, fermentation phenomena were to be regarded as a reflection of the life activities of microorganisms[2]; wherever fermentation occurred, there one would find a living organism. According to Liebig, ferments were substances in the process of decomposition, described as a type of chemical motion which could be transmitted to other, ordinarily unreactive,

[2] In his memoir on the lactic fermentation Pasteur wrote, "I have reasoned on the basis of the hypothesis that the new yeast [actually a bacterium] is organized, that it is a living organism, and that *its chemical action on sugar corresponds to its development and organization.*" [My emphasis, *T.L.S.*] Quoted by Conant (3), p. 32.

substances; the latter then underwent a characteristic decomposition. The opponents of the vitalist school at first denied the "living" nature of yeast, but as Pasteur went on to discover new micro-organisms and to relate known fermentations to them, the anti-vitalists were forced into the position, experimentally speaking, of attempting to extract the active ferment from the organism.

IV

At this time "ferments" signified agents responsible for a variety of chemical phenomena mediated by micro-organisms. That a single ferment might carry out each observed over-all process (such as alcoholic or lactic fermentation) went unquestioned. In 1878 W. Kühne introduced the term "enzyme" (or "unformed," "unorganised" ferment) for ferments whose action may be observed outside the living cell. The only examples available then were some hydrolytic enzymes. Those ferments which acted only within living cells were said to be "formed" or "organised"; they were considered to carry out oxidations and reductions, as well as cleavage of carbon chains and syntheses. Needless to say, Kühne's dichotomy of nomenclature merely restated the vitalist-mechanist controversy in other terms.

The first experimental evidence contradicting the unitary hypothesis of the causative agents of fermentations was provided by Emil Fischer and P. Lindner who worked with the yeast *Monilia candida,* a sucrose-fermenting organism. After some unsuccessful attempts to extract the "ferment" with water, they employed careful grinding of the fresh yeast cells with glass powder, followed by extraction of the broken cells. Their extracts now contained an enzyme (invertase) capable of hydrolysing sucrose to glucose and fructose (4). This separation of the yeast "ferment" into (at least) two parts might in itself have ended the controversy over the nature of ferments but for the fact that shortly afterwards Eduard Buchner reported that he had hit upon a procedure for obtaining a cell-free fermenting juice from yeast which could convert sugar to alcohol and carbon dioxide (2). Buchner's outstanding discovery, actually a turning-point in the development of modern biochemistry, did away with the vitalist argument that fermentations proceed only under the influence of intact cells, but at the same time it appeared to confirm the "one enzyme, one fermentation" theory. This assumption persisted for another decade, until Carl Neuberg described the carboxylase of yeast, a discrete enzyme separable from Buchner's yeast juice "zymase" (11-12).

V

By the end of the third quarter of the nineteenth century, it is true, some ferments, such as plant amylase and pepsin, had been isolated from their sources. But the crucial problem still remained of extracting the alcoholic ferment from yeast. Those investigators like Berthelot, Traube, and Hoppe-Seyler, who adopted an intermediate position in the vitalist-mechanist conflict, maintaining that there are also true "metabolic ferments" acting within the cell, made little headway with their views, regardless of how correct they are in retrospect, since they, too, were unable to accomplish the extraction.

A solution to a technical problem thus seemed a prerequisite for the dissipation of the pall of vitalism hanging over physiological chemistry; but it was urgently needed also as a basis for elucidating the numerous intimate chemical relationships within the cell, about which one could do little more than speculate, and to broach the fresh problems constantly generated by physiological investigations. As long as "ferments" resisted isolation from the cell, just so long would the corresponding activities of living cells continue to be interpreted in metaphysical terms of life-force or "protoplasmal residues." On the practical side, the difficulties inherent in attempting to study intermediary metabolic processes in intact cells acted as a brake on the progress of biochemistry.

This relation of theory to scientific achievement was consciously appreciated by Traube, and he had occasion in several of his papers to point it out. "A correct chemical theory of ferment action," he stated, "is, in general, an essential foundation of biological-chemical investigation (14). Although he had to defer to the realities of contemporary knowledge of fermentations, he was sanguine about the future of chemistry in this area. Referring to the failure of others to extract ferments by grinding or crushing cells, he explained that "the protoplasm undergoes a visible change [during such mechanical treatment] because in the cells it appears as a transparent slime, whereas in the press-juice it forms a flocculent curd. With this process of coagulation the protoplasm seems to have changed chemically, and with this change is bound up the cell's loss of alcoholizing power" (21).

Therefore, such failures should not be ascribed to the "vital" nature of ferments, as argued by the Pasteurians, but should stimulate researchers to try out new methods which will extract the unchanged ferments chemically. "Even if all decay processes depend

upon the presence of [micro-organisms], a healthy natural science would not permit [Schwann's] hypothesis to preclude further investigation. It would simply conclude from these facts that there are present in micro-organisms chemical substances which evoke the appearance of decomposition. It would attempt to isolate these substances and, if it could not do so without changing their properties, then it would only conclude that all means used for separation must have exercised a chemical influence on these very substances, changing them" (16). After all, Traube adds, we know the properties of a number of substances, "like the extractives, which we have not obtained pure, as well as the element, fluorine, which we have not been able to isolate" (16, p. 58). Yet no one, he added, would doubt the possibility of eventually isolating them in the pure state.

This emphasis on the physical and chemical individuality of ferments was corollary to the fact of cellular organization which, Traube considered, plays an important rôle in metabolism. Organization endows the cell with properties other than those observed in the crushed material. "The pressing out of the juices . . . is not a mere mechanical, but simultaneously a chemical, attack upon the substance of the fruit. In the cells, the individual substances are localized near one another in a definite way, and their mutual action is another matter than in the juice where they are mixed indiscriminately with one another (21).

When finally Eduard Buchner (2) succeeded in 1897 in extracting "zymase" (the system of enzymes and cofactors in yeast responsible for the alcoholic fermentation of sugar), and thus partially fulfilled Traube's programmatic aim of isolating ferments, there was provided a marked stimulus to research. For the first time it was possible to probe the intermediary stages of fermentation processes and to disclose the existence of many new compounds hitherto unsuspected, such as the phosphate esters and coenzymes.

VI

Traube's earliest scientific research, apart from that performed for his doctoral degree, was on metabolism. The dominant theory at the time was that of Liebig who argued that the function of proteins in the diet was to provide calories for work, whereas fat and carbohydrate supplied calories for heat production only, i.e., for maintenance of body temperature. Traube, along with his brother, Ludwig, and Heidenhain, physiologist at the University of Breslau,

had detected the errors in this hypothesis (18). It was Voit, however, who first published the critical finding in 1860 that muscular activity is not accompanied by an increase in urea excretion, as would be expected if Liebig were right. The next year Traube reviewed the whole problem of energy metabolism (18, 19) and administered the *coup de grace* to Liebig's theory. By comparing the heat of combustion of foodstuffs with the heat equivalent of muscular work, he was able to show that the portion of the diet contributing energy for muscular activity was in no way as small, in relation to the portion serving for temperature maintenance (i.e., resting conditions), as had been assumed for it. His contribution was soon forgotten, for in its wake came the spectacular observations of Fick and Wislicenus during their ascent of the Faulhorn, confirming the findings of the Voit School that exercise does not in itself cause an increased rate of protein metabolism.

The high rate of metabolism during physical work signified to Traube that the site of oxidations in the animal body was not in the blood, as was generally believed at the time, but in all tissues, primarily muscle. "I was the first," he recalled (17) twenty years after his work on energy metabolism, "to demonstrate on compelling grounds that the true center of the respiratory process is not the blood but the tissues of the body, and above all, the muscles; that the oxygen taken up in the lungs is liberated again from the capillaries, as dissolved gas, into the tissues of the individual organs, so that every single organ respires independently, at the expense of this free oxygen. Through this respiration the individual organs are in a position to grow, to function, to produce secretions and to develop the living forces characteristic of them, e.g., muscle and nerve forces (Kraft), etc.[3] This concept of tissue metabolism gradually infused physiology and established a basis for physiological-chemical investigation of intermediary processes. In a sense the picture Traube draws represents a physiological "atomism," for he has placed emphasis on the independence of both the respiratory activity and the specific function of particular organs from all others. Later studies have, of course, discovered the existence of nervous and hormonal integration of tissue metabolism.

[3] Traube ruefully noted that "although this doctrine has become one of the bases of the newer physiology, I do not find mentioned anywhere at all that it originated with me" (17). Keilin (8) assigns chief credit for this theory to Pflüger: ". . . The development of the idea that the respiration of an organism is localized not in its blood but within its tissues had a long and complicated course. It began with the work of Spallanzani published in 1807 and was completed only by the work of Pflüger (1872-75) who demonstrated that respiration is an intracellular process, a view which required another twenty years for consolidation and general acceptance."

In extending his concept of a metabolic process residing in the tissues to explain the origin of the characteristic "living forces," Traube maintained the primacy of biological oxidations but erred in details. For example, he assumed that the contractile substance of muscle was the mediator of the oxidation process in that tissue. "[My] analysis of the chemical processes of muscle respiration in essence is an application to the theory of slow combustion which I have developed [in my monograph]. It begins with the assumption that the muscle fiber, or rather the fibrin-like substance contained in it, is a vital decomposition ferment which transports the oxygen taken up in the blood to substances dissolved in the muscle fluid, without itself undergoing any decomposition."[4] In attempting to provide a material basis for muscular contraction which would be consistent with the physiological and enzymological knowledge available, Traube, it should be noted, retained the erroneous view, still current at the time, that each fermentation is caused by a single specific ferment. In his terms, the respiratory process in muscle is mediated by the contractile substance.

VII

Traube's studies on energy metabolism had logically led him to an appreciation of the metabolic process in the tissues. In turn, this raised the question of how metabolism is actually mediated; that is, how foodstuffs, so resistant to the action of molecular oxygen at ordinary temperatures, are oxidized in the body. His views on this subject can be considered under two headings: (a) his concept of ferments as chemical individuals, and (b) his theory of the mechanisms of enzymatic reactions. These were first expounded in 1858 (15, 16) and represent, as already indicated, a third alternative to the Liebig and Schwann-Pasteur views.

The essential content of Traube's theory was as follows: Ferments are chemical compounds, related to and derived from the proteins. Although not yet prepared in the pure state, nevertheless, like all other substances they possess a definite chemical composition. By manifestation of specific chemical affinities they bring about changes in other bodies without themselves undergoing any chemical change in the course of their action. From his experiments with diastase and starch solution Traube concluded (15, p. 58) that "each molecule of the passive body must come into direct

[4] Reference 18; pp. 403-4. In a paper (17; p. 663) which appeared several years after Danilewsky's discovery of myosin, Traube identified this protein, the contractile substance of muscle, as one of "the numerous oxidation ferments in the organism."

contact with the ferment in order to undergo conversion." Ferments are produced only by organisms but their action is independent of living matter. If decay ferments, for example, are produced "only in microscopic organisms, the mode of action of ferments does not cease, therefore, to be a purely chemical action. As shown by Musculus' discovery,[5] i.e., his extraction of the agent of urea fermentation (Ureagährung) from the organism producing it, the action of this ferment is not bound to any life-activity (14). In reply to the claims of the vitalists Traube exposed the source of their teleology by stating that "chemistry may well explain physiological processes, but physiology cannot clarify chemical events" (14). Indeed, he added, Schwann's thesis that fermentations depend upon the "life-activities" of micro-organisms should be inverted by stating, "Ferments are the causative agents of the most important vital-chemical processes not only in the lower, but also in the higher, organisms" (14, 16, p. 61).

His classification of ferments fell into two categories:

1. Oxidation ferments: these take up free oxygen and transport it to the substrate, whereby the latter is oxidized.
   (a) Vital ferments: found within the cells of higher organisms. E.g., the guaiac-blueing ferment of potatoes and other plants; hemoglobin; and myosin.
   (b) Decay ferments: in contrast to "vital" ferments these act on substrates lying outside of living cells. E.g., the ferment oxidizing alcohol to acetic acid in the quick vinegar process; the ferment oxidizing ammonia to nitrites in manures.
2. Reduction ferments: these split water.
   (a) Reactions in which there is no separation of products. E.g., the "fermentation" of urea to ammonium carbonate.
   (b) Reactions in which there is separation of the oxidized and reduced products. E.g., alcoholic fermentation.
   (c) Reactions in which no reducible group on the substrate is present; molecular hydrogen is formed. E.g., the fermentation of lactic acid to butyric acid, CO_2 and H_2.

[5] F. Musculus (*C. R. Acad. Sci. (Paris)*, 1876, *82*, 333) and later P. Miquel (*ibid.*, 1890, *111*, 397) reported the extraction of urease from bacteria. This enzyme does not catalyse a fermentation, but rather a simple hydrolysis. Thus Traube unknowingly made the same error as all his contemporaries were making, but employed it skilfully to buttress a correct theory.

Traube was largely responsible for recognizing the important rôle which water plays in enzymatic reactions. In oxidation-reduction reactions, he reasoned, water is split by the ferment, its elements being distributed to the reducible and oxidizable functional groups on the substrate (16, p. 105 ff). Until this time, Lavoisier's observation on the oxidation-reduction involved in alcoholic fermentation had been assumed to result from a "migration" of oxygen from one part of the molecule to another. In general, Traube's classification of ferments is based upon reaction mechanisms and represents an advance over contemporary classifications according to the extractability of ferments from cells.

Somewhat similar views concerning the rôle of water were held by Hoppe-Seyler, but he disagreed in respect to the existence of oxidation ferments. He assigned the central rôle in respiration to a hydrogen-releasing ferment similar to that in organisms causing decay processes. According to his argument, under anaerobic conditions these organisms form molecular hydrogen. In animals and plants, which exist under aerobic conditions, the nascent hydrogen immediately formed reduces molecular oxygen, producing hydrogen peroxide. It is the nascent oxygen then formed by the breakdown of the peroxide which is the immediate oxidant. According to Hoppe-Seyler, "respiration is nothing else than an act of decay in the presence of air."

To Traube this was a false mechanism, for animals and plants do not produce nascent or molecular hydrogen under any conditions, nor does the evolution of this gas occur in cadavers, where one might expect to observe the "ferment of putrefaction" make its anaerobic switch in end-products. Furthermore, since nascent hydrogen does not react with oxygen to form hydrogen peroxide, the postulated reactions could not occur. The actual mode of formation of peroxide was by the direct reduction of molecular oxygen in the presence of water and an appropriate metal.[6]

$$1.\quad Zn + \begin{matrix} \diagup H \\ O \\ \diagdown H \end{matrix} \begin{matrix} O \\ | \\ O \end{matrix} = ZnO + \begin{matrix} H-O \\ | \\ H-O \end{matrix}$$

$$2.\quad ZnO + H_2O = Zn(OH)_2$$

$$3.\quad Zn + H_2O_2 = Zn(OH)_2$$

$$Zn + H_2O + \tfrac{1}{2}O_2 = Zn(OH)_2$$

[6] Cf. ref. 17, p. 667. The differences of opinion between Traube and Hoppe-Seyler foreshadowed the later dispute between O. Warburg and H. Wieland. The question then was parallel: in biological oxidations is activation of oxygen or of the hydrogen of the substrate the primary event?

The frequently observed formation of hydrogen peroxide by living tissues was due to the presence in them of a reduction ferment decomposing water in an analogous fashion.

VIII

If Traube's contributions to the theory of metabolism and the rôle of enzymes have been largely forgotten, he is at least assured credit in the history of science for his discovery of artificial semipermeable membranes and for the enunciation of the first mechanical theory of (plant) cell growth. As is evident in his research on ferments as oxygen carriers and on the rôle of water in enzymatic reactions he often sought for mechanical models which might serve to clarify the workings of some physiological system. This interest, for example, impelled him to study the mechanism of autoxidation. Similarly, his investigation of plant metabolism led him to devise a model cell wall which would be pervious to certain solutes, but not to others (15, pp. 200-206; 207-212; 213-277). "M. Traube, as is well known," states Freundlich (6) "was the first to prepare [semipermeable membranes] artificially, when he showed that many amorphous films, such as are formed by difficultly soluble ferrocyanides, silicates, tannates, and so on, are to a large extent semipermeable; they allow water to pass through, but not, or to a much smaller extent, dissolved substances such as sugar, many salts and the like." Emil Fischer, in a brief eulogy on Traube (5) at a meeting of the German Chemical Society on 9 July 1894, pointed out that Traube's discovery had a twofold effect. It demonstrated first that the difference between colloids and crystaloids found by Graham, using animal membranes, is not a principal one, since Traube had been able to make membranes scaled in their permeability to a variety of crystalloids. Secondly, these artificial membranes made possible the later experiments of van't Hoff and others on osmosis and osmotic pressure, which were so important for the theory of solutions.

In working with his "precipitation membranes" Traube observed that the water and certain salts from the solution in which the membranes were suspended might pass into the spaces enclosed by the membranes.[7] A pressure was thus exerted which distended the membrane and increased the size of this artificial cell.

[7] Reference 15; p. 210. "Active membranes are thus atom sieves which can be used for the determination of the relative size of atoms." (Traube employs the word "atom" for "molecule," according to contemporary usage.) The plant physiologist Lloyd (9) dated the "ultrafilter or sieve theory" of permeability from Traube's work.

By transferring this simple phenomenon to biological material he enunciated his theory of cellular growth. "A process which depends both upon the formation of films and upon the semipermeability of the resulting membranes is the so-called artificial plant growth in the test-tube, which is noteworthy not only on account of its external similarity with living structures, but also because it must be taken into account in various natural processes" (6; p. 752).

IX

In reflecting on Traube's career one is struck by the fact that most of his time after his student days was spent in managing the affairs of the winery. In spite of this, he was able to carry on substantial investigational work and to adduce theories which have contributed to the advance of chemistry. His researches were in widespread fields: inorganic, organic, physiological, fermentation, and plant chemistry. Yet he was not a mere dabbler, choosing his subject matter wherever a profitable research might result. His work is concerned with the chemical and biological rôles of oxygen in the broadest sense: as an oxidant in the living cell and in the organic world. "It is, above all, oxygen that marvellous constituent of the atmosphere which supports all life on our planet, whose chemical properties and actions I have sought to trace. . . ."[8] "Truly, the story of oxygen comprises the story of organic life" (18; p. 414).

Traube's sense of responsibility to the paternal business left him a bystander of university research. Without students to continue his research problems and to publicize his doctrines in the scientific world, he is barely remembered as a pathfinder in the realm of cell physiology, permeability, and osmosis. In expounding his novel theoretical conceptions he had to contend with the prestige of Liebig, Pasteur, Hoppe-Seyler, and others, and for a provincial wine merchant this was a difficult problem.

But more. Traube began his scientific work when yeast had just been identified as a living organism, when scientific-experimental exigencies became prey to the tradition of vitalism, still surviving in biology and chemistry. Traube succeeded in grasping the facts known at the time which were pertinent to the problem and deduced their meaning in generalized and lasting terms, so that his participation in the fermentation controversy elevated him above those taking extreme and fallible positions. Thus he

[8] M. Traube in a letter to A. W. Hofmann; quoted by Bodländer (1).

recognized that living matter produces ferments but that these can be separated from and can operate outside of the cell. He regarded enzyme action as a chemical reaction between ferment (considered as a definite, proteinaceous compound) and substrate as did Liebig, but he rejected the untenable vagueness of the latter's theory of catalysis. Traube's work is marked by its scope and originality, making it well worthy of study for the history of biochemistry.

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A Medical Manuscript of the Fourteenth Century

LYNN THORNDIKE*

A FRESH glimpse of medicine in the fourteenth century is afforded by a manuscript at Munich written down or copied between December 1382 and January 2, 1387[1] by a Paul of Mugla (Muggia in Istria or Mügeln in Saxony?) or Paul Riether or Rietis of Mugla, as he also calls himself.[2] On July 11, 1383 he finished at Gorizia, where he had a salary from the count, a work begun at Mugla, where he also had a salary.[3] In March 1385 he copied several questions by Gentile da Foligno at Hewnfels or Hexenfels.[4] In July of that year he finished at Padua a treatise by Gentile which he had begun at Luentz[5] (Lunz in Austria?). Thereafter most of his copying was done at Padua, where he now began to lecture in medicine,[6] although once he speaks of himself as of Nürnberg.[7] Evidently he is to be identified with a Paul who received the doctorate in arts and medicine at Padua on April 18, 1384[8] and appears subsequently in the records of professors attending examinations in 1386 and 1387.[9]

In one instance Paul dates his completion of the work in question in the fourth hour of the night.[10] In another instance he

* Columbia University, New York, N. Y.

1 CLM 7609, on heavy paper in a small hand in double columns of about 67 lines to the column.

2 *Ibid.*, fol. 8 (6) rb, "scriptam per me Paulum in Mugla X.d. M.ccc. 82 mensis Decembris"; fol. 13 (11) va, "scriptum per me Paulum Riether Mugle a.d. M.CCC.83 die 17 Ianuarii"; fol. 15 (13) rb, "scriptum per Paulum Mugle a.d.M°CCC°lxxxiii ultima die Ianuarii."

3 Fol. 32 (30) vb: "Anno domini M.CCC.lxxxiii xi^{a} die Iulii finitus est hic tractatus de gradibus Gentilis in Gorizia me habente salarium comitis Meinhardi comitis Gorizie, et fuit inceptus in Mugla me habente ibi salarium."

4 Fol. 45 (43) rb, margin, "anno domini M.CCC. 85 in Hexenfels[?] per manus Pauli Rietis 16 marcii"; 46 (44) rb, "Finis questionis Gentilis per manus Pauli Rietis in Hewnfels anno domini M.CCC.84[*sic*] 8 Martii"; 46 (44) vb, "Gentilis a.d. M.CCC.85 Idus Martii in Hewnfels"; 48 (46) ra, "Finis questionis Gentilis a.d. M.CCC.85 21 Martii in Hewnfels per manus Pauli." I have been unable to locate Hewnfels.

5 Fol. 55 (53) ra, "Padue finita a.d. M.CCC.85 3^{a} Iulii et in Luentz inchoata per Paulum."

6 Fol. 79 (77) rb, "Paulus semper Padue M.CCC.85 tertia Decembris in vigilia Sancti Nicolai anno primo sue lecture medicine."

7 Fol. 61 (59) va, "per me Paulum Rieter de Nurumberga [*sic*] 28 Octobri M.CCC.85 Padua." This antecedes the "Padue semper" of the previous note.

8 *Monumenti della Università di Padova (1318-1405)* raccolti da Andrea Gloria, II (1888), p. 171.

9 His father is called Conrad of Nürnberg, and Paul is usually called "de Nurimbergo," though once (p. 193) "Paulo de Allemania," but I did not see in the records themselves the name, "Paolo Richer da Norimberga," which Gloria (*ibid.*, I, 414) applies to him.

10 Fol. 90 (88) vb, "quam scripsit Paulus Rietis M.CCC.85, 19 Decembris 4^{a} hora noctis."

says that it was finished on the 21st of February "in vigilia sancti Petri cathedrati." This shows that that feast was not instituted by Paul IV in 1558 or 1556 but merely restored (as of January 18), and indicates that it fell on February 22 rather than February 23.[11] He adds that, on the day before, the hosts of de la Scala invaded Paduan territory and burned Salvazanum and many other villas. On completing one treatise on March 6, 1386, he notes that it was Shrove Tuesday,[12] and, in the case of the next treatise, that March 9 was the third day of Lent.[13]

Most of the texts that Paul copied were by Gentile da Foligno, who had died nearly forty years before in 1348. The first in the volume, however, was of a determination by Angelo of Siena in the University of Bologna,[14] copied apparently by Paul in Mugla. It discussed the question, which had already been treated in the *Conciliator* of Peter of Abano (*Diff.* 80), whether all the arteries were dilated and constricted at the same time as the heart.[15] Dilation and constriction of the heart and arteries was also the subject of the second text copied by Paul, a defense by Gentile da Foligno of the accepted view that it was caused by the soul, against the argument of Thurisianus or Torregiano of Florence (called Plusquam Commentator from his work, *Plusquam commentum*), that it was a humid ebullition and foaming which made the heart expand, and that constriction occurred when the fervor weakened and the vapor expired.[16]

The third text copied by Paul was by Francesco Zanelli, a physician at Bologna in 1347 who died in 1365. Zanelli gave an affirmative answer to the *Dubium*, whether the spirits of the body are formally animated, because he believed the soul to be the

11 Fol. 124 (123) ra. See R. T. Hampson, *Medii aevi Kalendarium*, p. 173.

12 Fol. 128 (127) ra, "Scripsit Paulus Rietis Padue a.d. M.CCC.86 6ª die Martii et tunc erat carnis privium."

13 Fol. 130 (129) rb, "Paulus scripsit Padue M.CCC.86 nona Martii et erat teria dies in quadragesima."

14 No Angelo of Siena appears in the faculty lists of the University of Bologna as published in 4 vols. by Umberto Dallari, *I rotuli . . .*, but the lists extant date only from 1370 on.

15 Fol. 3 (1) ra. "Quesitum fuit in questione generali an supposito quod omnes artarie simultanea dilatatione dilatentur et simultanea constrictione constringantur, ipse constringantur ad constrictionem cordis et dilatentur ad dilationem cordis . . ." Fol. 8 (6) rb, "Angelus de Senis in Bonon. studio determinavit hanc questionem . . ."

16 Fols. 8 (6) va-13 (11)va: "Thurisianus Florentinus dictus Plusquam Commentator subtiliter conatus est tenere quod dilatatio et constrictio cordis et artarictarum non fiat ab anima sed quod propter ebulitionem spumificantem humidum cordis et elevantem in humorem fit dilatatio et quod constrictio fit cum fervor obtunditur et vapor expirat . . . / . . . Hic igitur finitur defensio opinionis medicorum de motu cordis contra Plusquam Commentatorem secundum Gentilem. . . ."

The opening words of the standard treatise by Galen, *De iuvamentis membrorum*, are, "Corpora animalium sunt instrumenta animarum que in eis sunt. . . ."

activation of an organic physical body potentially having life, and that spirits bring virtue to the members of the body in the form of heat, by which the operations of the soul are exercized.[17]

Next comes the work of Gentile on degrees of medicines in which he asks whether they are definitely measured by comparison to the temperate body or by comparison to every man or to man absolutely (*ad omnem hominem sive ad hominem absolute*). The opening words given for this work in *A Catalogue of Incipits,*[18] "Ad investigationem ergo [here *igitur*] scientie de gradibus . . ." are preceded by an address of a few lines to "Amice karissime magister Grissianine . . ." and the whole concludes piously at fol. 32 (30) vb:

Et debemus credere quod deus gloriosus cuius nomen sit benedictum iuvit posteriores nostros et iuvabit nos in acquisitione scientie et notitie qualitatum et quantitatum omnium vel plurimarum medicinarum, et quas res non possumus cognoscere via scientiali dabit nos gratiam comprehendi illas per bonitatem estimationis faciendo homines ultimi bonitatis in iudicio estimato et intellectu.

Various questions by Gentile follow: the first, concerning the conservation of health, discusses whether the human body in an abnormal condition, say choleric, should be conserved by likes or by contraries.[19] He next inquires whether there is any such thing as disease pure and simple.[20] Then it is questioned whether natural complexion and that of one's time of life differ,[21] and whether *calidum naturale* acts upon *humidum radicale.*[22] A problem which

17 Fols. 13 (11) va-15 (13) rb: "Dubium utrum spiritus sit formaliter animatus, quod sic quia anima est actus corporis physici organici potentia vitam habentis . . . / . . . et isto modo dicimus quod spiritus defert virtutem ad membra, id est, quemdam modum caloris quo exercentur operationes anime. Laudetur omnipotens. Franciscus de Zanellis Bon."

18 Lynn Thorndike and Pearl Kibre, *A catalogue of incipits of medieval scientific writings in Latin,* Cambridge, Mass., 1937, col. 21.

19 Fols. 33 (31) ra-45 (43) rb: "Dubium an corpora lapsa ut in exemplo colericum corpus debeat conservari per consimilia vel per contraria . . . / . . . Ex cuius regulis et dictis de conservatione temperati corporis poteris accipere canones in conservatione cuiuscunque corporis. Et licet canones possis habere, tamen materias unicuique corpori mensurare est difficile secundum unamquamque rem. Quilibet tamen secundum suam estimationem conetur." Another MS is Wiesbaden 60, 15th century, fols. 40vb-54, "Queritur an corpora lapsa ut in exemplo . . ."

20 Fols. 45 (43) rb-46 (44) rb: "Est questio utrum sit reperire distemperantiam simplicem . . . / . . . Et ideo oportet quod variatio mona (?) harum oppositionum non possit fieri manente omnimoda in varietate in alteris. Utraque tamen duarum oppositionum habet propriam intensionem et remissionem sicud habent propriam entitatem et propriam contrarietatem. Pro solutione alterius rationis videbis in solutione alterius questionis de humoribus vel de sanguine."

21 Fol. 46 (44) rb-vb: "Est questio utrum complexio naturalis et etativa differant. Videtur quod sic primo quia Avicenna et Haly Abbas et alii faciunt distincta capitula de complexionibus naturalibus scilicet de modis temperamentorum et de complexionibus etatum . . . / . . . tamen sibi non constituit hec duo duas complexiones in actu."

22 Fols. 46 (44) vb-48 (46)ra: "Est questio utrum calidum naturale agit in humidum radicale . . . / . . . et ad tertium dico quod forma non agit sed subiectum per formam."

many have discussed before Gentile is when a disease reaches its height. In concluding he mentions an experience of his own in which a continuous fever kept getting worse until the crisis was reached. It was "perfect" by discharge through the nostrils and urine.[23]

The next three questions by Gentile are as to medicines: whether new theriac is stupefying,[24] whether drugs which purge by compressing may be mixed with those which purge by softening, such as mirobolani and cassia or viola (a question previously discussed by Dino del Garbo),[25] and whether excessive pulverization of medicaments makes them ineffective.[26] Both of these last two *dubia* were drawn from Gentile's commentary on the second book of the *Canon* of Avicenna.

The question, whether any variety of dropsy results from a diseased liver alone,[27] is followed by another (addressed to a very dear friend, Thomas of Arezzo) on whether so-called potential medicines are activated by the heat of our body and, if so, how. This was finished by Gentile at Perugia on April 17, 1342.[28] He had previously composed a brief tract on the subject,[29] but Thomas had requested a fuller discussion of it, in the course of which Gentile notes that we are heated by pepper but chilled by the juice of the poppy.

23 Fols. 48 (46) ra-55 (53) ra: "Est questio in quo tempore morbi sit maior morbus, ut febris putrida salubris in quo tempore sit fortior sive intensior, et maxime utrum in tempore status in quo materia est magis digesta. Et multi scripserunt in hoc quesito ante nos sed nota[?] complebimus sermonem in hoc . . . / . . . Hec sunt que in hac materia vidimus secundum nostrum posse, nec debet quis negare sensata pro una difficili ratione. Fateor enim in me incepisse quemdam febrem continuam que usque ad crisim in malitia semper augendo processit. Crisim autem habuerit perfectam per saniem narium et urinam. Gentilis de maioritate morbi."

24 Fols. 55 (53) ra-57 (59) rb: "Est questio utrum teriaca nova sit stupefactiva . . . / . . . In nomine domini questio Gentilis finita Padue M.CCC.85, 23 Octobris in domo."

25 Fols. 57(59)rb-59(61)ra: "Dubium utrum possimus commiscere medicinas que evacuent comprimendo et que evacuent leniendo ut mirobolanos et cassiam vel violam . . . / . . . Ex quibus omnibus iam apparet solutio argumentorum difficilium Dyno, et credo quod Dynus a longe speculabatur ea que diximus."

26 Fol. 59 (61) ra-vb: "Dubium utrum superflua trituratio medicinarum destruat operationem earum . . . / . . . non omnis trituratio est suspecta, sed debes triturare sic quod salvetur forma et debes dare totam medicinam trituratam in sua dosi. Sed supposito quod des partem, non agit, quare," etc.

27 Fols. 59 (57)vb-61 (59) va: "Dubitabile est an quelibet species hydropsis possit fieri solo epate nocito . . . / . . . et tu considera hec. Gentilis in causis hydropsis."

28 Fols. 61 (59) va-70 (68) ra: "Amice carissime, magister Thoma de Aretio, sollicitasti me multum ut preter brevem tractatum quem composui de reductione medicinarum ad actum prolixe et clare in forma questionis rescribere, quod libenter facio. Dubitatum igitur fuit semper utrum medicine que dicuntur tales in potentia reducantur ad actum a calore nostre corporis et quomodo . . . / . . . Gentilis de Fuligno Perusii M.CCC.xlii xvii Aprilis." Other MSS are Venice, S.Marco VII.11, 14th century, fols. 44-48; Wiesbaden 60, 15th century, fols. 30v-40va.

29 I do not know if it is extant.

The treatise of Gentile on resistances or the contraoperation of members of the body was composed by him in February 1341, and Paul Rietis finished copying it at Padua in 1385 on the eve of most blessed Katharine, virgin and martyr (November 24), in the third hour of the night.[30] It also occurs in another manuscript, now at Erfurt, of various medical works copied at Vienna and Prag in 1393-1394.[31]

The next question by Gentile in our manuscript, whether fever arising from phlegm alone is more extended, longer, and slower in its course than fever arising from phlegm mixed with choler,[32] appears to be identical with the first of several questions by him on the duration of fevers, which are found in one of the manuscripts at Erfurt mentioned in the previous note.[33]

Of Gentile's question regarding heat in youth and boy[34] I know of no other manuscript, but that on hunger and thirst[35] is found in Vatican Latin MS 4445 at fol. 206 and is alluded to in the printed *Summa medicinalis* of Tommaso del Garbo (Venice, 1506, fol. 50va),[36] where Tommaso writes:

Hoc quesitum sub hoc titulo invenimus disputatum a Gentili in quodam tractatu de fame et siti quem fecit: et illum habemus incompletum quantum de eo fecit tempore quo sub eo audiebamus medicinam. Nescimus

30 Fols. 70 (68) ra-74 (72) va: "Resistentia sive contraoperantia membrorum adinvicem sive in tertio est maxime per qualitates complexionales . . . / . . . tunc calor epatis superveniens subiciet se illi ut materia forme sue et sue perfectioni. Laudetur deus. Tractatus Gentilis de resistentiis M.CCC.41 de mense Februarii. Paulus Rietis semper Padue a.d. M.CCC.85 in vigilia beatissime Katharine virginis et martiris tertia hora noctis."

31 Erfurt, Amplon. F. 251, fols. 196v-201: "Resistencia sive contraoperancia membrorum . . . / . . . subicit se illi ut forme et sue perfectioni. Expl. scriptum Gent. de Fulgineo de contraop. memb." Earlier in the same MS is Gentile's *Summa de urinis* at fols. 151-181: "Sicud dicabatur superius, significationes principales . . . / . . . ad libros particulares et lege litteram etc. Finitus est iste liber in Wienna in cena Domini 94°." Also in Erfurt, Amplon. Q.233, anno 1403, fols. 16v-33. Quoted from Schum's *Verzeichniss.*

32 Fols. 74 (72) va-79 (77) rb: "Dubitatur an febris de solo flegmate sit prolixior sive longior et tardior in motu quam febris de flegmate mixto colere . . . / . . . Questio Gentilis de prolongatione febris composite."

33 Amplon.Q.233, fols. 1-16: "Queritur utrum febris de solo fleumate . . . / . . . prolongari contigit quandoque et hec est subtilis solutio, consciam ergo illam; hec de questione. Expl. quest. de prolongacione et duracione febris flegmaticarum et febris terciane . . . G. de F. speculatoris sublimis, D. gr. Gentilis." Quotation from Schum.

34 Fols. 79 (77) rb-86 (84) ra: "Questio fuit proposita generaliter utrum calor in iuvene et in puero sit equalis in radice . . . / . . . laudetur Omnipotens et beatissima virgo Maria iuvantes me lete frui hac et al aliis. Gentilis."

35 Fols. 86 (84) ra-90 (88) vb: "Fames et sitis sunt dispositiones satis sensate de quibus non est clare determinatum a philosophis naturalibus . . . / . . . hoc est compilatio eorum que occurrunt in cognitione famis et sitis causarum ipsarum subiectorum[?] et finium secundum Gentilem de Fuligno."

36 At fol. 114rb of the same edition, in connection with various interpretations which have been made of a passage in Avicenna, Tommaso, who did not die until 1370, says of the relations between his father Dino and himself, and Gentile da Foligno (died 1348): "Alie sunt expositiones quas dedit Dynus et sunt de manu sua in marginibus nostri libri quem ipse notavit, quod etiam a nobis habuit Gentilis."

tamen si postmodum eum complevit: non enim vidimus ulterius, in quo ipse premittit quod in stomacho est duplex nutrimentum.

He then quotes Gentile verbatim for 45 lines.

The next item in our manuscript, Gentile's commentary on *De differentiis febrium* of Galen,[37] was printed at Lyons in 1514, and is found in another manuscript later than ours.[38]

Not ascribed to Gentile and not expressly stated to have been copied by Paul is the next anonymous discussion whether motor activities are innate or acquired.[39] Then we come to another question by Gentile, whether health can be acquired by art.[40] Other questions by him follow on the use and meaning in medicine of the words, *simpliciter* and *nunc*,[41] and on the relation of different kinds of fevers.[42]

Of the few remaining questions in our manuscript I had time to examine only two.[43] One was whether the operations which medical men say are produced by specific form or occult virtue are produced by substantial form or by some accident consequent to form or composite.[44] The other, composed at the request of Antonius de Abscisio, is whether solution of continuity is *per se* and immediately the cause of pain.[45]

The contents of our manuscript are in large proportion unfamiliar, although the themes with which they deal were not

37 Fols. 91 (89) ra-112 (110) rb: "Incipiunt ea que recollegit Gentilis super de differentiis febrium. DIFFERENTIE FEBRIUM, ETC. Galienus vir mirande laudis natione fuit Grecus de civitate que dicitur Pergamum . . . / . . . Finis Questionum quos collegit Gentilis super differentiis febrium." The questions are listed in a "Registrum contentorum in hoc volumine," on one of the unnumbered leaves at the beginning of the MS.

38 Wiesbaden 57, 15th century, fols. 274-358.

39 Fols. 112 (110) rb-118 (116) va: "Virtutes anime quibus operantur membra esse omnes innatas vel omnes influentes vel quasdam innatas quasdam vero influentes . . ." The rest of 118 (116) v and the next unnumbered leaf are left blank.

40 Fols. 119 (117) ra-124 (123) ra: "Utrum sanitas possit fieri per artem . . . / . . . Laudetur deus, finitur sermo de sanitate Gentilis."

41 Fol. 124 (123) ra-va: "Ad sciendum quomodo extra communem modum dicendi possint intelligi hec nomina simpliciter et nunc in sano et egro oportet pre (?) intelligere. . . . / . . . hec intelligit commentator in commento Gentilis."

42 Fols. 124 (123) va-128 (127) ra: "Dubitatur utrum febris putrida sibi coniungere effectum febre . . . / . . . Hec est investigatio compositionis generum febrium adinvicem per Gentilem."

43 The library closed unexpectedly early on Saturday at 11 a.m.

44 Fols. 128 (127) ra-130 (129) rb: "Queritur utrum operationes que a medicis dicuntur fieri a forma specifica que vocantur operationes occulte fiant a forma substantiali dante esse vel ab aliquo accidente consequente formam vel compositum . . ."

45 Fols. 148vb (the last digit of the other numbering had been cut off) -153 (151) va: "Carissime magister Antoni de Abscisio credebam prolixe scribere in causis dolorum, ut me scripturum rogasti, sed multa occupato . . . An solutio continuitatis sit per se et immediate causa doloris . . . / . . . Nos autem credimus quod proportio sit qualitas proportionata et sic dicit[?] in proportione Gentilis."

uncommon then. The questions by Gentile da Foligno in particular further enlarge the bibliography of that very industrious and voluminous medical author and add new material to previous accounts of him.[46]

[46] One of these is the author's *History of magic and experimental science* (1934, vol. III, chap. XVI, pp. 233-252).

Felice Fontana: The Laws of Irritability

JOHN FELIX MARCHAND*
AND
HEBBEL EDWARD HOFF**

Part III. Philosophical Research on Animal Physic

In 1775, somewhat over a year after the appearance of the first treatise on the laws of irritability, a large paper-bound folio volume, *Ricerche Filosofiche sopra la Fisica Animale,* was printed in Florence by Gaetano Cambiagi, Stampatore Granducale. Fulfilling his promise, the author in the new book ventures to expand his principles of irritability into the widest field of animal economy and to banish with forceful argument all the difficulties proposed against them.

The work is in two parts: one on the laws of muscular irritability and the other an examination of some of the more obscure points of animal economy. Part I is an Italian version of the original dissertation, *De Irritabilitatis Legibus.* It reproduces Laws I-V in the original form. The old Latin Part II, *"On the inefficacy of the animal spirits in muscular movement,"* is not a law. It now appears in the new book as the last half of the first chapter in Part II.

Amid Fontana's numerous subsequent publications there were no other editions of either book after the appearance of the Latin version in book form at Lucca in 1767, but in 1785 a German translation of *Ricerche Filosofiche sopra la Fisica Animale* was published by Dr. E. G. B. Hebenstreit, Ausserordenlicher Professor der Arzneikunde in Leipzig, entitled *Felix Fontana's Beobachtungen und Versuche über die Natur der thierischen Körper,* [etc.][37]

An English translation of Hebenstreit's German version has been made and this text has been compared with the rendering from the original Latin treatise. The separate translation from the German has made it possible to check the Latin, the Italian, the German, and the two English texts against each other in search of all possible discrepancy.

The Italian is found to follow the Latin treatise with only the most minor changes, for instance, the addition to footnote (a) in

* New York, N. Y.
** Houston, Texas.

Chapter I of a remark that the tame squirrel slept on Fontana's own bed. In the more concise Hebenstreit translation this long footnote is shortened and incorporated in the body of the text. The material concerning the old Law III of the Latin edition is given

RICERCHE FILOSOFICHE

SOPRA

LA FISICA ANIMALE

DI

FELICE FONTANA.

Tomo I.

IN FIRENZE L' ANNO MDCCLXXV.

PER GAETANO CAMBIAGI STAMPATORE GRANDUCALE

CON LICENZA DE' SUPERIORI.

Fig. 4. Title-page of Fontana's later expansion of his Laws of Irritability and refutation of a vital principle, the "animal spirits," published in Italian in 1775.

paragraph by paragraph in both the Italian and the German versions, except that both omit the concluding passage.

Fontana's dedication of the work on Animal Physics to his friend Haller and the editor's preface to this work are not carried

over into the German. Aside from these minor differences the essential points of the argument are found to be faithfully reproduced in the German edition.

(1)

PARTE I.

DELLE LEGGI

DELLA

IRRITABILITA MUSCULARE

CAPITOLO I.

LEGGE I.

Ad ogni contrazione della fibra muſculare è ſempre neceſſario un nuovo ſtimolo, che ne riſvegli l' irritabilità.

§. I.

NOn ſarà certamente impreſa facile, come forſe taluno potrebbe immaginarſi, l'eſaminare le leggi di quella nuova proprietà della fibra animale, che dall'Hallero, il quale è ſtato il primo ad illuſtrarla con nuove, e belle eſperienze, è chiamata irritabilità Halleriana. Imperciocchè niente di più ſi è ſaputo finora, ſe non che la fibra animale ſtimolata che ella ſia, per un'inſita proprietà, come ſi crede, ſubito ſi contrae.

a Qua-

Fig. 5. First page of *Ricerche Filoſofiche sopra la Fisica Animale,* stating in Italian Fontana's first Law of Muscular Irritability.

With Laws I and II from the Latin treatise the following pages incorporate the additional Laws III, IV, and V from *Ricerche Filoſofiche sopra la Fisica Animale,* together with Fontana's dedication to Haller and the editor's preface.

TO SIGNOR BARONE ALBERTO DE HALLER

The reading of your immortal work of physiology, Friend and most Venerated Signor, has awakened in my heart the desire to experiment on my powers of investigation and analysis of some of the more hidden phenomena of animal physics. Your magnificent discovery, the light of the philosophical genius which emanates from every page of your distinguished works, has opened to me a path for new research. If the fortune really be granted me to set foot in some unknown realm of human knowledge, you will have been the inspiration and the example for it. Would that I might always find myself on the same level with you; but see also in the book, which I offer in tribute to you, after how much difficulty and discussion, and with what veneration and respect I ventured to hold an opinion sometimes different from that of a *Haller*. The love for truth of men as great as you is the second of the passions, if not perhaps the primal passion unknown to the vulgar soul, in whom the feeling of personal weakness disturbs serenity and security at every least indication of modest philosophical discrepancy. This same love for the truth will serve as the center for the union of our spirits in our diversity of opinion.

The honor, then, which in so many parts of your work it has pleased you to pay to my trifling productions, has inspired me to make you this dedication, among the many other titles which will be devoted to you.

[FELICE FONTANA]

TO HIM WHO READS

Here is a work, Courteous Readers, which perhaps without my eagerness would never have seen the light, at least until much later. It was condemned to oblivion for many years by its Author, and was buried beneath some of his other manuscripts and pages which he called rancid, and which his continued occupation with work would not permit him to finish. To me has fallen the task of making him look over this first tome, and he has done it to please me.

I then believed I should join to this first tome *The Laws of Muscular Irritability* published more than a year ago in Latin for our Author by the publishing house of Lucca. Without these laws never would the present work have been interwoven as a happy application of these same laws to the more difficult phenomena of the living mechanism. Whoever reads this work without the spirit

of having participated, but with the enlightenment which is necessary for a good understanding of it, will suddenly discover that irritability is no longer a vague and uncertain hypothesis but a demonstrated truth and that these laws fixed by our Author are everlasting sources for the understanding of the more obscure motions of the animal.

It was necessary to demonstrate that the nervous fluid was not the active cause of muscular motion, and it was necessary to discover the laws of this new property of the fiber. Both these ends were accomplished in this work. The first assures us of the truth of the principle, and the second the fecundity of it. One without the other would have been a step towards the truth, but sterile and infertile; it would have been like gravity before Galileo, attraction before Newton. It is not sufficient to know that an active principle exists in nature; it is necessary to know its properties, and the laws that it observes, to apply it with certainty to phenomena. Since the time of Aristotle it was known that light passing through water was dispersed, but the exact laws that it observed in dispersing are due to Newton's Optics and the Spyglass of Dolland.

The Author decided that the first essay had pledged him to give the remaining part of this voluminous work, of which what I have been able to raise up again from MS will be at least five tomes in quarto. The second tome deals with voluntary and involuntary movements of the animal, where among many new interesting things is given the reason for all the surprising operations of the somnambulists in a manner so evident, and so certain, that there is perhaps no motion in all the animal mechanism which is recognized and understood better than this.

The third contains an immense series of observations and reasoned experiments on animals divided in many parts, and on the feelings and passions that followed to conserve the same parts, even though detached from the animal.

In the fourth there is added to the subject two new attributes, one a tendency, the other entirely active and always in action, from which in the end is derived the tendency of the bodies. This is the most sublime and delicate part of the work, with the greatest utility and application to all physics.

The fifth embraces the animal motions, both natural and nonnatural, reduced to the sole two principles pointed out above.

A moment of reflection, for one who knows animal physics and the general physics of the body, will suffice to make him see the

high importance of this work, and what a new world of unexpected truth we can expect. Certain it is that the work is of a fundamental nature and original, as this first volume has already demonstrated in part.

[THE EDITOR]

CHAPTER III

Law III

A muscle contracted for a long time loses its irritability

I. The third law of irritability of animal fiber concerns muscles which, when they have remained contracted for a long time, have for some reason a completely or partly lowered aptitude for movement.

II. If the muscles of the hands, arms, feet, legs, or any other part of the body are held contracted for a long time, we feel ourselves forced to relax them: nor can we immediately move them again as before, but rather must expect that while recovering the original disposition for voluntary movement we either cannot move, or can only do so with the greatest difficulty and loss of force. Concerning this anyone can obtain good guidance from himself, and in his movements will often have proved it.

III. I have placed various animals, such as dogs, cats, and goats, in uncomfortable positions so that many of their muscles had of necessity to remain in strong contraction. I palpated the tension of their contracted limbs and the hardness of the muscles. When some time had passed and the animals were given their liberty, they could no longer right themselves, but fell to the ground or in their fright would struggle along the ground to escape.

IV. Over a long time I kept the legs of some decapitated frogs contracted by a simple electric current applied with a point of iron to the vicinity of the spinal medulla. After I removed these irritated beasts from the electric device they gave not a single sign of the lively movements I perceived in other frogs decapitated at the same time but not electrified. They no longer made leaps, such as the non-electrified ones could do. When I stimulated either the spinal cord or crural nerves, the legs of the electrified frogs moved much less than those of the non-electrified. In short, it always seemed to me that the electrified muscles contracted much less than those which were not electrified.

V. When I stimulated the diaphragm on one side in many warm-blooded animals, and irritated the spinal marrow in many warm- and many cold-blooded animals, especially frogs, the legs would remain contracted as long as I continued to prick the spinal cord with the point of a long needle, and the diaphragm partly lost its irritability, moving very little after being touched with the point of the iron, while the other side where the nerve had not been touched moved quite well. Moreover, the legs of some animals, on whose spinal cords the iron point had been placed, moved much less when their nerves were stimulated than those of others in which the cord had not been stimulated.

VI. All these and a thousand other experiments I could adduce to prove more conclusively, if you wish to be sure, the certainty of this third law of the animal, that a muscle contracted strongly for a long time loses its irritability, which it does not recover quickly, nor does it return to its function or its original state and condition until an interval of time has passed.

VII. The experiments carried out in II show that we cannot, at least without great fatigue and exertion, contract or maintain in contraction the muscles which we previously moved so easily if we have held them contracted for an interval of time. And therefore even if the exciting cause has not ceased (which surely would not be the case), it does not fail to produce a smaller movement of the muscle. Therefore the muscle has lost some fundamental aptitude and facility for contracting in response to stimuli; the irritability is lacking; therefore a muscle contracted for a long time loses this necessary property at least for some time.

VIII. The same truth is easily recognized from consideration of Paragraphs III, IV, and VI, in which it is seen that any muscles which have been held in contraction over a long time, or with great force or irritation of the nerves or spinal marrow either by electricity or by a hard sharp body, are deficient in that aptitude for motion which they had shortly before.

IX. I do not wish to deny that occasionally we seem obliged to relax muscles contracted over too long a time because of pain which we experience, and at other times we feel constrained not to move as we please for the same reason. But this only follows after excessively strong contraction or from actions carried over too long a time, as on a long trip. With smaller contractions the pain becomes entirely negligible, and nevertheless we feel the im-

possibility of moving, at least for a long time, and if we really succeed in doing it, it is not without great exertion and fatigue and expenditure of energy; very shortly we feel the most extreme langour and desist. If we desire to overcome this unpleasant sensation, the muscles refuse to obey, or move poorly and weakly, more from lack of irritability in the fiber than from lack of nervous fluid or from pain which one may overcome by effort of the will.

X. The pain which we feel when muscles are held contracted with too much force or for too long a time arises from violent or too long continued pressure on its fibers. The delicate nervelets of the contracted muscle must suffer great pressure similar to what a heavy body would make on the muscle. It appears also that the same cause which occasions pain in the muscle must also render it less irritable. When the muscles contract powerfully or move over a long interval of time their component fibers cannot fail to be greatly distorted, crowded, unequally distributed, and distorted. A state of violence of the fiber is necessarily contrary to the natural state of the muscle, which is relaxation, and in which irritability appears more vigorous and strong; therefore it is not surprising that this aptitude is lost with motion. Distortion in this way must follow in the fiber when the movement of the muscles is longer or greater than their structure in the irritable state will permit.

XI. Contracted muscle relaxes not only because the animal wishes to be free from pain which it is suffering, but also because the fiber has lost its original irritability. With the irritability lost it is necessary for the animal, if it wishes to move, to assail the fiber with an increase of spirits in the muscle, and then indeed even against his wish the muscle will relax. Thus does it come about that the eyelids will not remain open for a long time, but close in spite of us. This great fatigue proves that the animal has contracted his muscles too powerfully over too long a time. It then becomes necessary for a greater quantity of nervous fluid to be sent to the muscle and made to increase the stimulus on the fiber when it is less irritable, and thence from excessive consumption of spirits it finds itself compelled to rest and remain at rest.

XII. I do not intend to exclude always the various other causes which also in some peculiar circumstances, such as illness, can take effect principally in rendering the animal incapable of motion. Too great an expenditure of animal spirits in a painful disease or

long and arduous mental concentration can well render the muscles slower and less able to move. But who will deny that there has not also occurred a change in the fiber such that it is less irritable than before? Certainly disturbances can arise in the animal fluids which must be communicated to the muscles. And also from diminution of the amount of this nervous fluid in the muscle can the fiber not lose some of its irritability? It is certain in any event that the nervous fluid maintains in the relaxed muscle a certain natural tone and tension through which it is more apt to move when stimulated.

XIII. No proof at all in favor of the animal spirits is the sudden acquisition of strength and a greater propensity for movement in the entire body after the drinking or smelling of spirituous substances, since at most through this is demonstrated an increase in activity, and perhaps in animal spirits, which is equivalent to saying that the effect comes about more readily with increase of the stimulus to the fiber, which is one of the circumstances under which a muscle contracts. Why would a muscle not move from a much stronger stimulus, if it has not yet lost all of its irritability? Moreover we do not know in what way these spirituous substances act on the animal mechanism, how quickly they can be carried to the fibers, or what changes and disturbances they provoke.

XIV. Excessive expenditure of animal spirits may indeed come about from vigorous and sustained movements of the animal. But we would have established our law even without these cases, and might suppose that only a few muscles were in motion or held themselves contracted, or even a single one. From the motion of a few muscles, and more particularly from that of one alone, consumption of the spirits could certainly not have taken place, yet these muscles still lose their irritability. In such instances there is no improvement, or very little, from revival by the use of aromatic and spirituous substances.

XV. The player of the pianoforte moves his fingers with great rapidity for a whole hour without becoming fatigued and without the muscles of the hand losing their aptitude for motion. The eyelids move all day, the diaphragm never rests, and with all this the animal is not fatigued. The consumption of animal spirits by the player of pianoforte in keeping in action so many muscles for so long a time, and by ourselves in raising and lowering the eyelids, and the amount devoted to expanding the thorax is without doubt

greater than this consumption which occurs in holding a single muscle contracted for a few moments. Clearly in this case the animal cannot cause it to move only because it is no longer as irritable as before.

XVI. And here opens out a wide vista for interpretation of animal economy and the nature of disease. With the most vigorous exertion of the living mechanism a paralysis appears, either completely or in some part where the movement was greatest. Most uncertain is recourse to animal spirits, defect of a viscus, or injury to the nerves when the seat of the ailment is nowhere else but in the muscle fibers themselves. Their component parts are evidently distorted in these movements of violence and no longer possess that order and disposition which renders them capable of irritation; therefore they can no longer move, and perhaps can never recover, if the alteration they have undergone is sufficiently forceful and violent. In vain does one have recourse to medicines, shown by experience itself to be fruitless. If there is to be any cure in such cases it will be necessary to treat the muscle, rather than the nerve, which has probably suffered no harm.

XVII. Paralyses can therefore occur through defect of the muscle alone, particularly in certain parts, without involvement of the nervous fluid. This is certainly so in great weakness of all the body; and of those illnesses in which there is principally attack on the strength of the animal in such a way that it can no longer stand and use its muscles, the principal cause is the loss of irritability of the fiber. Fevers of the army, and of the jail, scurvy, malignant fevers, pestilences, the plague itself, and all the putrid fevers are among this number. In the muscle fiber, then, originates this great alteration in the movements of the animal, and the decreased frequency and weak pulsation of the heart shows most evidently that the muscular system is attacked, because this is a muscle which does not recognize any other stimulus or other cause of its movement than the venous blood which continues to be brought to its ventricles even in such grave illnesses as those above.

XVIII. It is commonly believed among doctors that convulsions arise from too great an abundance of nervous fluid which in some way irritates the fiber of the muscles. But the truth is that originally those who are weaker are also more subject to this malady; nor does it seem plausible that from their brains should be dis-

tilled a greater quantity of animal spirits than occurs in persons who are healthier and stronger and those who are better nourished.

Those who are prostrated from much hemorrhage, and especially women who after childbirth have suffered great loss of blood, are observed to be subject to the most severe convulsions and will perform such movements as they could never have accomplished in health. The greatest convulsions of the living body are observed in animals prostrated by loss of blood, and these are the greater in proportion as the animal is more debilitated and practically exsanguinated. In such cases it would be quite ridiculous and also contrary to all reason to revert to too great an abundance and too great an impetus of the animal spirits where there is occurring a diminution in blood and circulation of humors.

This delicate equilibrium of forces and tension from fiber to fiber and from muscle to muscle which is present in those who are well, and which is maintained in the muscle by a certain proportionate quantity of humors which they ordinarily receive, is lacking in the body mechanism in disease.

All muscles, even when they are relaxed, preserve a certain degree of tension in all their fibers, through which they balance against the antagonistic muscles and remain as if at rest. This truth is demonstrated by cutting some of the fibers of a muscle, after which these cut fibers at once retract toward the fixed ends and leave a large aperture.

Besides this it is observed that if a muscle is paralysed, its corresponding antagonist suddenly of its own accord shortens and contracts and comes together, although the fiber of the latter is not stimulated more than usual by any new influx of fluid.

The grosser fluids in the animal body and the nervous fluid all function together to bring about this tension in the muscles and maintain the motor power of the animal in a nice equilibrium. In the muscles which are convulsed the fluid is wholly or partially lacking or irregularly lost and diminished, there never being supported a proportionate equalization of loss of humors in such parts. Where the circulation is so unequal it happens that the diminished fluid within the fibers comes out irregularly. Gone also is the equilibrium from muscle to muscle and to fiber to fiber. Hence the tremors and convulsions.

XIX. What we have just said about convulsions will furnish the solution for a great difficulty brought up by the men who would

attribute the motion of muscles to the nerves, because the imbalance in the animal which has lost much blood arouses in it such convulsions as would never have occurred without this excessive loss.

In these cases the convulsions arise from loss of equilibrium between muscle and muscle, since blood is then distributed unequally in the various vessels; thereupon these vessels empty with so much more disorder and inequality, the loss of blood is so much more extensive and more instantaneous, that they cannot so suddenly contract with their natural equality and harmony; therefore the muscle fibers become stretched or compressed to a greater or lesser degree; therefore there awaken extraordinary movements and vigorous contractions. But one sees actually that even great loss of blood, when it has come about more slowly, does not occasion any motion in the muscles, or not as much, since it provides opportunity for the vessels to constrict equally and distribute their humors with greater uniformity.

The fact that all this movement truly arises from destroyed equilibrium in the muscle fiber appears clearly in an experiment which I have repeated many times.

In a given muscle, for example, one on the neck attached at one end to the skull and hanging at the other, it is possible to excite movement when it is not present and increase it when it is there, if the muscle is sectioned in such a way that part of its fibers are cut and the rest are intact, in short by bringing it about that some of the fibers are no longer under any tension, as they had been previously, and thus the usual equilibrium between them is suspended. And from this it is found that the muscles of the neck or some other part contract with greater force and for a longer time when they are alone and separated off than when they were attached to the part at both ends.

XX. Even the peristaltic movement of the intestines and that of the stomach are now explained, although the stimulus applied to their fibers always continues to irritate them. Thus the circular fiber contracts from this stimulus, the lumen of the intestine is constricted, the contracted circular fibers instantly lose their irritability and relax, then longitudinal ones are stimulated by the feces and shorten the intestine. And successively the one losing and the other acquiring irritability as the stimulus becomes more or less active and more or less close to the fiber, there follow these

movements of shortening, lengthening, constricting, and enlarging throughout the entire canal, movements the more difficult to determine, even for the most keen-eyed observer, since ordinarily, with both the longitudinal and the circular fibers being stimulated irregularly by varying actions of the stimulus in various portions of the canal, they become disordered in their movement, and these motions which are so discordant in one part come into opposition with motion in another, and thus the irregular motions of constriction and dilatation are perpetuated in the long canal.

XXI. There remains still to be explained a very singular phenomenon, and one which I observed for the first time with my esteemed friend Sig. Caldani in Bologna. The intestines, which appear to move even in the living animal as seen when the abdomen is opened, suddenly developed lively and powerful movements at the time of its death and continued to do so most surprisingly for a whole hour. At the end of 1756 I showed that this singular phenomenon at the death of the animal was common also to the stomach and the whole oesophagus, and I was able to demonstrate that the blood which in the living animal distends the vessels of these parts impedes these movements of the intestine, movements which arise suddenly when the vessels become empty either through the death of the animal or through injury, so that it is possible to produce these violent movements also in the living animal. But how indeed can it come about that when their vessels are empty of blood, the intestines, stomach, and oesophagus become so vigorously moved? This is what I shall now explain, if I may.

We have seen already that all muscles in the living animal possess a sort of tension and that they do not move because their contracting forces are in mutual equilibrium. This condition of equilibrium in the fibers of the muscle is maintained also by the blood, so that by lack of this fluid the equilibrium is upset and interrupted from muscle to antagonist and from fiber to fiber in the same muscle. This movement must be more active in the intestines for several reasons. Besides the presence of two contrary and opposite orders of fibers there is also the continued and unequal stimulation of the feces. Moreover, the muscle fibers are loose, as it were, free of any restraint over long distances, and without proper attachments. The intestines therefore are in no way different from any of the other muscles because of the singular character of this motion, since it merely arises from the peculiarity

of their structure, which is such that the movements must be relatively vigorous. In conclusion, then, there occurs in them as in all other muscles a convulsion when the animal's vessels become empty of blood, and the convulsions are occasioned, as in all other parts of the animal body, by destruction of the equilibrium of the forces in the muscle fibers.

Hereby also is explained a singular accident of the gravid uterus which heretofore has remained most obscure to all physiologists, or has been assigned only vague and unfounded hypotheses. Drawing off of blood induces abortion in many instances, and a simple washing of the feet or hot bath may also do the same. In these cases there occurs an unequal distribution of blood in the various parts of the uterus which is already swollen and distended with humors, since through a thousand circumstances easy for an anatomist to see the dilated and distended vessels of this organ then empty or fill more unequally; this upsets the equilibrium from part to part and fiber to fiber, unequal contractions necessarily follow, most powerful convulsions, and many other disorders which finally produce abortion. The muscular fiber of the uterus, as long as it is in equilibrium with others, cannot induce a single movement of the uterus, but no sooner is this equilibrium between muscle and muscle lost than powerful contractions begin through action of their natural irritability.

What I have said above on movements of the animal body produced by loss of equilibrium throws light on the agitated question of the cause of natural birth. An infinite number of causes have been imagined by the anatomists to explain natural birth and the reason that delivery of the gravid uterus follows a determined interval of time. I am inclined to believe that even natural birth comes about from loss of equilibrium and from irritation of the fibers. The muscle fiber of the uterus becoming intolerant of too much distension, the unequal pressure produced especially by the head of the fetus against the cervix of the uterus, humors which no longer spread with equal ease through uterine vessels which no longer fill equally due to the unequal pressure of the placenta and fetus, all contribute to the disorder and upset the equilibrium of the uterine muscles; therefore irritation and movement is communicated from one to the other, severest pains are excited and accomplish the birth.

CHAPTER IV

Law IV

A muscle stretched or compressed for a long time loses its irritability

I. The fourth law of irritability of animal fiber concerns muscles which are forcibly stretched, or are compressed for too long a time, or crushed by weight applied externally to the fiber.

II. I injected tepid water through the urethra into the bladder of a live cat so that it filled completely and stretched like a balloon. Shortly afterwards, having opened the lower part of the abdomen, I found it still filled and the walls tightly stretched. I stimulated it with sudden needle pricks and electricity, but it did not move in response to these stimuli, although I had emptied out the water. It was flaccid, rather enlarged, and without motion. The same experiment I repeated on a kid, two lambs, and a dog, with the same result in all. No more irritability was discovered in the bladder after the injection. I likewise forcibly injected with tepid water the bladder of a large dog which I then kept alive in the room. It was no longer able to urinate; and the water had to be removed with the syringe. A great difficulty in urinating persisted thereafter for all the days that I observed him, which were many.

III. With an anatomical siphon I forcibly injected venous blood at natural temperature, or tepid water, at one time into the right ventricle of the heart, ligating the pulmonary artery, at another into the left, ligating the aorta, and the same was done with the two auricular appendages of this muscle. I consistently observed that when these muscular cavities were expanded excessively they lost their aptitude for motion and when stimulated would no longer contract.

IV. I injected tepid water through the pylorus into the alimentary tract of a cat in which I had opened the lower abdomen, and continued until the canal was firmly distended with water. When this was emptied out suddenly through the stomach, the peristaltic movement of the intestines could no longer be seen, nor did they contract any more when stimulated. They had become more flaccid than usual, and also their capacity was greatly increased. In another cat I injected only the large intestine, which likewise I found without motion when the animal was opened.

The same experiment I performed on various other animals, injecting also the oesophagus and the stomach. All these parts which I injected with violence lost their natural motion and irritability.

V. I placed the hearts of various animals, particularly cold-blooded ones, in a glass receptacle, raised the pressure with a mechanical device, and as the air in this receptacle became more dense the hearts of these animals beat less and less frequently until the motion finally ceased altogether in consequence of the compression. Taken from the receptacle they showed that they had little or no irritability even when I stimulated them with the sharpest needles. The same was observed with other muscles, and a decapitated and skinned viper suddenly lost all its movement in air thus condensed. The hearts of some frogs I put into a flexible balloon filled with tepid water, and this I compressed forcibly between the hands and the knee, after first wrapping it with linen cloths to make the pressure more equal. When these hearts were taken from the balloon, they moved little or not at all; the heart and various muscles of other animals I pressed between two balloons filled and distended with air, and these, like the ones in the other experiments, would suddenly lose entirely or in part the aptitude for moving.

VI. The experiments which we have just described establish the truth of the fourth law of irritability in a manner so evident that no further doubt can survive. From repeated experiments it is seen that in every case in which the fibers of a muscle are compressed or stretched forcibly they lose motion and the aptitude for contraction in response to stimuli which constitutes the actual irritability of the animal fiber. The most powerful pressure and stretching cause a dislocation and separation of the molecules in the muscle; and the filaments which compose the fiber lose their more intimate texture, whence their order is disturbed and the cement substance which binds them together loosened, so that the whole organization of the muscle is altered and loses its irritability.

VII. Thus is explained why the bladder becomes paralyzed and no longer in condition to express urine from the body when in some disease it is kept distended for too long a time. Those who for the purpose of convenience forcibly retain their urine for a long time are exposing themselves to the same danger. Likewise an excessive abundance of blood brought to the heart, or difficulty in

expelling it in ordinary life, can deprive this muscle of its irritability and give rise to organic and incurable disease. The more vigorous efforts of the body, or fright, or excessive joy, all can rob the heart of its irritability and kill unexpectedly. In such cases there arrives in this muscle all at one gush a great abundance of blood with which its cavities are filled, dilated, and distended beyond their usual limits and the heart, no longer able to distribute the blood because of this accumulation of too great an abundance in its ventricles, exhausts its own walls in striving to contract, and thus loses irritability and movement.

VIII. It is also observed that when placed in a vacuum the heart and other muscles immediately cease moving and lose irritability. Air within the muscle evolves when that surrounding it is removed, and it separates and distends the components of the fiber in such a way that it at once loses its original irritability. The stomach too much distended with food or air and the excessively dilated intestines may easily lose their irritability, and from this various diseases may result.

Tympanites, through great and unusual distension of the intestines, take away their irritability so that in the absence of their normal peristalsis they can no longer compress and expel the air against them so that they become more and more distended and weakened. One helps such cases most by restoring or increasing irritability, as is accomplished by the more powerful stimulants, not without advantage to the patient.

It must certainly seem strange that the gravid uterus can become so distended without its muscular fibers losing their natural irritability. Actually it is observed that when the foetus is not quite out it contracts with the greatest force and then returns almost immediately to its original natural state. This will suddenly cease to be such a marvel if one will reflect that the distension comes about by such gentle and successive degrees that the fibers do not suffer this strain all at once, but have time to become lengthened little by little without being broken or their internal structure damaged.

The part is also distinguished from the other harder and more compact muscles of the body by its greater softness and flexibility, such that the more delicate and perhaps more abundant cement substance can give way and stretch more easily without dislocation of the primary components of the elementary fiber. The principal increase when the uterus is gravid occurs especially in the venous

channels, which grow large and become filled with a larger quantity of humor and which also are like the muscle fibers in being distributed as a network in such a way that they can give way more easily without suffering strong tension.

IX. Even cold water deprives muscles of their irritability. I immersed the hearts of many animals in very cold water and also in cold blood, and consistently observed that they entirely stopped moving, and that they would immediately begin to move less in proportion as the water was colder. I put decapitated, skinless vipers into very cold water, and they at once ceased to move although the muscles of these animals ordinarily contract for a long time in water which is not so cold. Muscles thus placed in very cold water at once lose not only all motion, but also the faculty for ever recovering again. Excessive cold, therefore, is absolutely destructive to muscular irritability and gives rise to paralysis in the muscle.

Very cold water injected into the intestines of a living animal can render them paralytic, as I have observed on many occasions in cats and dogs. These very cold clysters must be used in medicine with great reserve, and only when it is desired to relieve powerful convulsions of the intestines or when the lower muscles are too active or too contracted, and where there is no fear of inflammation.

Water which is too hot also robs the fiber of its irritability, perhaps because its dislocates and separates the molecules composing the muscle or because it fixes and coagulates the cement substance; hence it should be taken with great reserve by mouth in order that the stomach will not be left debilitated.

CHAPTER V

Law V

A muscle which remains relaxed for a long time loses its irritability

I. This law concerns muscles which have remained relaxed for a long time through illness or through some other circumstance.

II. Those who for some reason are obliged to remain in bed for a long time no longer have the obedience of their muscles as before; the weakest movement fatigues them, and they sometimes arrive at the sad necessity of no longer moving at all.

III. If an arm or leg is tied up for some days in such a way that the blood can circulate freely, this member becomes no longer obedient

to the will, and if it succeeds in moving at all does so only with much difficulty and for a short time.

IV. It is observed also that when ordinary movements are kept restrained and inactive in this way, they either do not take place as easily as before or do not take place at all, and after we have striven to move for some time, they become even less willing to contract than before.

V. Daily experience demonstrates this truth. It is therefore certain that the muscle loses its irritability if it is not used and if it remains for some time in its natural state of relaxation. I do not believe we wish to return to the nervous fluid rather than altered organization of the muscle fiber to explain this impotence for movement, since it does not seem plausible that the nature of this fluid could be altered in a healthy and well-nourished animal. And certainly one will not believe those who would say that the molecules of the nervous fluid are altered in such a short time or that the nerve itself is obstructed, for such causes are pure imagination and supposition.

VI. I am inclined to suspect that many of the muscles of the animal are unable to move more through lack of irritability than through whatever other circumstance you may wish to imagine, such as perhaps that we do not know of the organs so that we could order animal spirits to them. There are, for example, the muscles of the external ear which we do not know how to move and which in spite of our efforts decline to contract. Yet there are said to be men who know how to move even the ear, and certainly the other animals move it very well although it is certain that their muscles are stronger and more numerous. Ever since childhood, however, we are unaccustomed to these movements, since we have never felt the need to move the ear, so when we desire to move it after we have become adults it is no longer in our power, since the muscles of this part, being relaxed and without use for such a long time, have lost their irritability, and we vainly command the animal spirits to contract them. But those animals which derived some sort of comfort and frequently found themselves in need of moving the ear and therefore formed the habit of moving this muscle have preserved its irritability. One could perhaps accustom a man to move this part if frequently even from infancy one would tickle and prick these areas and he had no avenue of escape from this molestation except motion of the ear. He would then prove, if you

wish, that even at the end one would certainly succeed with difficulty and only after a long time, since these muscles are in truth very small and one could not move the part very much, and finally it appears that babies themselves for this same reason generally have difficulty in moving their muscles.

VII. And consider the weakness in which we find ourselves after a profound sleep. It even seems that we no longer possess the limbs and that the muscles which worked with such ease a short time before no longer know how to move. In vain the animal spirts are ordered into the fiber, the muscles continue to remain relaxed. It seems clear that this results entirely from the fact that irritability is either lost or greatly diminished during the period of sleep. The fibers of the muscle, too long relaxed in sleep, have lost that disposition without which there is no longer any irritability. A greater abundance of nervous fluid is then capable of shaking up the fibers and restoring them to their original condition. If a dog is observed at the moment of awakening, one sees him utterly listless, incapable of righting himself, and falling down again by his own weight. This generally occurs also in all other animals. The ordinary motion then returns to the muscle through a vigorous shaking of the fiber or by rubbing which will remove from the muscle the torpidity in which it finds itself either in deep sleep or in illness, proving that in this condition the muscular fibers were altered.

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Pierre Sergescu 1893-1954

GEORGE SARTON*

THE International Academy of the History of Sciences was founded in Paris in May 1929, largely because of the obstinate devotion of the Italian historian, Aldo Mieli (1879-1950). Mieli was its first general secretary and was succeeded in 1947 by the Romanian mathematician, Petre Sergescu.

Sergescu was born on 17 December 1893 at Turnu-Severin, a place of great historical and poetical interest. Its Romanian name does not ring bells in the memory of western people, but let them simply remember that it is a deformation of the Latin name Turris Severi.[1] The place was originally called Drobeta but took its later name from a tower built to commemorate Septimius Severus.[2] Turnu-Severin (to return to its present name) is in Oltenia;[3] it is a river port in one of the most picturesque reaches of the Danube, just below the Iron Gate, a gorge two miles long. One can still see nearby the ruins of the bridge built by Apollodōros (II-I) of Damascus in 103-05 under Trajan; it was the largest bridge of the Roman Empire.[4] The memory of such events must be a source of inspiration; another source was the agreeable white wine of that district and the abundant rose gardens which perfume the atmosphere. I am recalling these pleasant facts because it will help us to imagine the place where our friend was born, where he spent his youth and received his first education.

He continued his studies at the University of Bucharest (1912-16) where he prepared simultaneously for his graduation in philosophy and his graduation in mathematics. Even that was not enough to satisfy his ambition; he was also taking courses at the Conservatory of Music from which he managed to graduate at

* Cambridge, Massachusetts.

1 Various Roman places were called Turris . . . (tower, castle), e.g., Turris Stratonis, Turris Hannibalis. Compare the names of many European cities beginning with Castel, like Castel Gandolfo, Castellammare, Castelnuovo, or Château; English names beginning with Castle, Arabic names beginning with Kasr (Qasr), like Louksor on the Nile, al-Qasr al Kabir (Alcázarquivir) in Spanish Morocco, Alcazar de San Juan in Spain.

2 Septimius Severus (emperor 193-211) brought back into Dacia the Legio V macedonica, and established various camps, notably in Drobeta. He expired at York in 211 at the age of sixty-five.

3 Little (or western) Walachia, in southwestern Romania.

4 It was 1119 meters long and was supported by twenty arches and twenty-two pillars; the superstructure was in wood. There is a statue of Trajan in the main square of Turnu-Severin, and remains of the so-called Severus Tower can be seen in the city park. These are remains not of a Roman tower, however, but of a mediaeval one (thirteenth century?).

about the same time.[5] His various activities were stopped by World War I. He had exerted influence to bring Romania in the war on the French side and when this occurred in August 1916, he was permitted to enlist as a cadet in the corps of engineers (le corps du génie). At the beginning of 1917, he was made prisoner by the Germans and interned; he was obliged to spend eighteen months in a prisoners' camp in Bulgaria.

After the armistice on 11 November 1918, he was liberated and obtained a fellowship for study in Paris, where he arrived in the following year. He attended courses at the Faculté des Sciences and the Ecole Normale, prepared the French "licence," and began his work toward the doctorate. He was called back to Bucharest by his master Traian Lalescu (1882-1929) and obtained his doctor's degree. In 1924 he was appointed assistant professor at the University and the Ecole Polytechnique of Bucharest, and in 1925 was professor of mathematics at the University of Cluj, the chief city of Transylvania.[6] During this first period of teaching, he found time to compose various mathematical papers and started the journal *Mathematica* under the patronage of two older Romanian mathematicians, Dimitrie Pompeiu and Georges Tzitzéica (both born in 1873). From 1929 to 1948, twenty-three volumes of *Mathematica* were published, partly in Romanian and partly in French. Not only was Sergescu the managing editor and one of the main contributors, but he devoted large sums of his own money to the undertaking. *Mathematica's* purpose was mainly to encourage mathematical studies in Romania, but it welcomed foreign contributions, especially French, and therefore was well known in mathematical circles abroad.

Until 1930 Sergescu devoted all his time to mathematics; but in that year, under Mieli's influence, he became interested in the history of science, and that interest grew until the end of his life.

The eighth meeting of the Academy was scheduled to take place at Bucharest in 1936; Mieli and Sergescu organized excursions across the country, festivities and receptions in many places. This congress was very successful, for it not only awakened Romanian interest in our discipline but also revealed one of the

[5] This suggests a comparison with Raymond Clare Archibald, for whom see my biography in *Osiris* 1956, *12*.

[6] Cluj, in northwestern Romania, was always the main city of Transylvania, but in earlier times it was better known under the German name Klausenburg or the Hungarian name Kolozsvar. It was there that Matthias Corvinus (Matyas Hollos) illustrious king of Hungary (1458-90) was born in 1440, the second son of Hunyadi Janos; the house where he was born still exists and is occupied by a museum of ethnography.

Pierre Sergescu, 1893-1954

most beautiful countries of Europe to the foreign visitors.[7] This revelation to them of the loveliness of his native country was for Sergescu a great source of happiness.

His first contribution to our studies was included in the *Tableau du XX^e siècle* (Paris, Denoël et Steele, 1933; *Isis, 23,* 539). He outlined the history of mathematical sciences in our century (182 pp.); his account was focussed upon the French achievements with which he was most familiar.

His work was interrupted by World War II; his experience of the first war was partly repeated. Again he tried to bring his own nation close to the western side. When the Hungarians invaded Transylvania, he was obliged to leave Cluj and was successively professor at the Universities of Bucharest and Timisoara;[8] in 1945, he was elected professor at the Ecole Polytechnique of Bucharest and later became its rector.

In 1946 he went to Paris to give a series of lectures and did not realize that he would never be able to return to his beloved country. He took an increasing part in the organization of the Academy and Union.[9] At the Fifth International Congress held at Lausanne (September-October 1947), he was appointed general secretary (executive officer) of both organizations. From then on, all of his time was given to them and to the history of science in general. For example, he was one of those who helped to create the mathematical section in the Palais de la découverte. He helped to arrange the first hall, devoted to the history of numbers, a series of lectures on the history of science, and special exhibitions devoted to Pascal and Leonardo da Vinci. Many of the lectures were given by himself; the last one was to be devoted to Three Historians of Science, Tannery, Duhem, and Sarton.

Together with a few friends, Sergescu helped to found in Paris the *Fundatia regala universitara Carol I* (Institut universitaire roumain Carol I) and the *Bulletin scientifique roumain,* volume 3 of which is now in the course of publication. He was the president of that Romanian foundation until the time of his death.

At the Seventh International Congress in Jerusalem (August 1953), I was able to witness his activity but also, alas! his growing

7 Aldo Mieli: Un viaggio in Romania (*Archeion,* 1936, *18,* 191-203).

8 Timisoara (Temesvár in Hungarian) is the chief city of the Banat which the Treaty of Trianon (1920) divided between Yugoslavia and Romania. The Banat is north of the Danube and west of Transylvania.

9 The distinction between the International Academy of the History of Sciences and the International Union *ditto* is explained in my *Horus* (1952, 253-55).

illness and fatigue. I was sensitive to that because I was myself tired and ill but did not have to discharge heavy responsibilities as he did. His resilient energy and courage were marvelous; in spite of fatigue and pain, he did his duty and managed to be cheerful. At the end of the congress, he fell so ill that one wondered whether it would be possible for him to sail back from Haifa to Marseille.

After his return, his illness became gradually more ominous, and he suffered a stroke at the beginning of 1954; he was obliged to spend three months in hospital and after his return home was slowly recovering his strength. In the night of 21 December 1954 he passed away during his sleep.

It is impossible to speak of him without paying tribute to his wife, who shared the good days and the bad days of his life and the poverty of exile. She is of Polish birth and a distinguished author, writing in Polish and in French, under the name of Marya Kasterska. They experienced poverty and discomfort together but never lost courage and dignity.

Sergescu was a very generous man, generous to the core, and his good wife helped him to be more generous. They were faithful Catholics, and their religion made it easier for them to weather many calamities. As all their belongings were lost when they refused to acknowledge the new communistic regime in Romania, they were very poor but were never ashamed of their poverty. They were exceedingly hospitable to friends, and it was my privilege to enjoy their simple but gallant welcome when I visited them in their little home in the rue Daubenton no. 7, close to Saint Médard, a famous old church, at one end and to the mosque and Muslim institute on the other, close also to the Jardin des Plantes where they could stroll in lovely surroundings whenever they needed a rest.

Sergescu was very modest and kind, always eager to be of service to others. We all know people who try to puff themselves up by speaking contemptuously of others; he was the opposite of that and was genuinely great in his simplicity and poverty.

* * *

The Académie has been flourishing thus far because of the disinterested passion of two men, Mieli and Sergescu. Let us hope that a third man of their kind will soon be found to continue and complete their mission. Institutions are not created or kept alive in their youth by dry administrators but only by consecrated men like Sergescu and Mieli. The vicissitudes of early academies are easy to

imagine; one has only to think of the old Accademia dei Lincei (Rome, 1603-30) and of the Accademia del Cimento (Florence, 1557-67). Other academies, like the Royal Society (London, 1662) and the Académie des Sciences (Paris, 1666), were more fortunate in that their activities were never interrupted, yet their beginnings were far from easy. International academies such as the one of which we are speaking are running new risks caused by differences in nationalities, languages, customs and manners, and their integration is made more difficult, but they express one of the healthiest and most hopeful aspirations of our age.

Science is above parties, sects, and nationalities; it is not only international but supernational; the history of science should be investigated not to increase sectional vanities but rather to establish more clearly the real purpose and the glory of mankind. That is what Mieli and Sergescu tried to do; we must continue their efforts and reach a better fruition of them.

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Sergescu wrote a great many papers in French and Romanian, which were published separately or in Romanian, Polish, and French periodicals. It will suffice to say here that most of his historical papers dealt with the history of mathematics in general, the history of French mathematics, the history of science in Romania. Many of them were analyzed or listed in *Isis* soon after their appearance.

An elaborate bibliography of his writings is in the course of preparation and will be eventually published in Paris.

Notes and Queries

Edited by Dorothy M. Schullian, Ph.D.*

Two Association Copies of the FABRICA

The Stanford University libraries possess two copies of Vesalius' *Fabrica,* a first and a second edition, which by reason of the inscriptions upon their title-pages have a unique association, respectively, with the anatomist's family and with one of his distinguished contemporaries.

The first copy to be noted is the first edition of 1543 purchased from the Roman bookseller C. Lang on that fateful 1 August 1914. It did not reach the University Library, where it is still housed, until 30 September 1915. This copy is not large, and it may be assumed that it was cropped to its present size by whatever seventeenth century binder provided it with its present full leather binding. Nor is this copy in good condition since some of its illustrations are lacking; indeed, it can be termed shabby through use. The explanation of this condition is possibly to be found in the inscription on the title-page (Fig. 1) which appears to read: *In usu[m] Coll[egii] Soc[ietatis] Jesu Ingol[stadii] ex liberalitate do[nato]r[is] Viduae Vesalii . . .*, that is, "In the possession of the College of the Society of Jesus in Ingolstadt through the generosity of the donor, widow of Vesalius. . . ." The final word has been thumbed into obscurity.

Vesalius had married Anne Van Hamme, the daughter of a civic official of Brussels, and at some time after the death of Vesalius his widow married one Henri Van der Meeren. This may or may not suggest that the use of the term "widow" reflects a presentation of the volume in that period between the death of its author in 1564 and the remarriage of his widow, but it does not explain how or when this particular copy of the *Fabrica* reached Ingolstadt.

Ingolstadt in Catholic Bavaria was one of the major German academic defenses against Lutheranism. It was John Eck, vice-rector of the university, who successfully debated against Luther in the first years of the new religious movement and thereby unwittingly assisted the Saxon reformer to recognize the necessity of a complete rupture from Rome. As an aftermath of the Council of Trent and the revival of Catholic enthusiasm in Germany, Ingolstadt, supported by the Fugger interests and under Jesuit influence, passed to the offensive, and with the association of such powerful Jesuits as Salmeron and Canisius it became the chief academic center of the Jesuits in Germany and an impulse to the dissemination of Jesuit schools throughout that land. In 1800 the University of Ingolstadt was transferred to Landshut, a move indicated in the case of this copy of the *Fabrica* by a

* Cleveland, Ohio.

CVM CAESAREAS
Maiest. Galliarum Regis, ac Senatus Veneti gratia & privilegio, ut in diplomatis eorundem continetur.
BASILEAE. 222279

Fig. 1. Vesalius, *De humani corporis fabrica*, 1543. Stanford University Library.

CVM CAESAREAE
Maiest. Galliarum Regis, ac Senatus Venet. gratia &
priuilegio, ut in diplomatis eorundem continetur.
BASILEAE, PER IOANNEM OPORINUM.
Ioachimo Ioach. Fil. Camerario.

Fig. 2. Vesalius, *De humani corporis fabrica*, 1555. Lane Medical Library.

circular stamp bearing the words *Ad Bibl[iothecam] Acad[emiae] Land [esutensis]*. Presumably books hitherto possessed directly by the Jesuits had upon the dissolution of their order become the property of the university. Finally in 1826 the university was removed to Munich, but since the present volume bears no mark of the University of Munich or of its library, it may have been during the course of this removal that it passed into private hands whence after the passage of about a century it has again taken up academic residence.

The second inscription on the title-page: *Tertii ordinis,* that is, of the third or tertiary order, constitutes a puzzle. It does not appear to be written in an old hand and, of course, immediately suggests the Franciscans, although in time other religious organizations came to have similar orders. Is it a reference to Vesalius' widow, is it a suggestion that the anatomist himself had become a tertiary in some religious order, or is it a reference to something completely outside the present context, possibly, as has been proposed, a library classification?

The second copy of the *Fabrica* to be noted is in the Lane Medical Library. It is the second edition of 1555, and it is a noble copy complete in every detail and still in its original binding of stamped pigskin on oak boards; thanks to its escape from the binder's knife, the page measures 43 by 29 cm. The title-page (Fig. 2) of this copy bears two inscriptions. First: *Joachimo Joach[imi] Fil[io] Camerario,* that is, "[The book of] Joachim Camerarius, son of Joachim [Camerarius]." Thus it appears that this copy once belonged to Joachim Camerarius, 1534-1598, physician and botanist, who had studied philosophy under Melanchthon, medicine under Crato von Kraftheim, and eventually received his degree of doctor of medicine at Bologna in 1562. After his return to his native Nuremberg and some years of medical practice he persuaded the city fathers to establish a school of medicine in 1592, of which he was the dean until his death six years later. While the chronology of his life and activities prevented Camerarius from knowing Vesalius, yet as a friend of Falloppius he may well have had a considerable indirect knowledge and interest in the great anatomist. The phrase "son of Joachim" is a reference to the physician's father, Joachim Camerarius, 1500-1574, a celebrated German humanist, friend of Luther and Melanchthon, professor of rhetoric at Tübingen, and later the reorganizer of the University of Leipzig. It is quite possible that the book was a gift from father to son—certainly a fitting gift to a young medical student—and the inscription, which is in the hand of the son, would admit of that interpretation. In that case, however, filial regard should have led the younger Camerarius to record the source of the presentation.

The second inscription: *Harles Dr. Erl.,* must be that of Johann Christian Friedrich Harless, 1773-1853, of Erlangen, a distinguished physician and medical historian. Unfortunately, there is no indication as to how the book passed from the family of Camerarius to Harless, nor how thereafter it came

into the possession of another historian of medicine, Ernst Seidel of Oberspaar, Saxony, whose entire collection, including this volume, was purchased for the Lane Medical Library in 1921.

CHARLES DONALD O'MALLEY

John Snow—Epidemiologist

The Medical Officer for 27 May (p. 285), 15 July (p. 28), and 22 July (p. 46) reports the celebration of the centenary of publication by Messrs. J. & A. Churchill of the second edition of John Snow's epidemiological classic *On the mode of communication of cholera.* The observance, which had been appropriately arranged by the Section of Epidemiology and Preventive Medicine of the Royal Society of Medicine with the co-operation of the London School of Hygiene and Tropical Medicine, took place on 17 May, at which time a commemorative exhibition of Snow, arranged by two D. P. H. students, Miss Lorimer and Dr. Rochester, was on display in the Museum of the School. The two principal addresses were by Professors James M. Mackintosh and Bradford Hill, the latter this year's President of the Royal Society of Medicine Section and Acting Dean of the School. Professor Mackintosh's colorful address was read by Dr. J. H. F. Brotherston and gave a vivid picture of the social background of Victorian England and of the circumstances in which Snow was led to carry out his studies on the spread of cholera.

Professor Mackintosh pointed out that Snow himself was not exactly "a clubbable man," since he was a bachelor, a teetotaler, and a vegetarian. Some twenty-five years before the causative organism of the disease had been recognized, he had shown by close clinical observation the way in which cholera was spread. In Broadwick Street, somewhat east of Regent Street and not far from John Hunter's Great Windmill Street Medical School where Snow had studied, there was a pump which, in 1854, had spread an epidemic of cholera through sewage contamination of its water. Professor Bradford Hill in his address expressed the opinion that Snow's pump was probably less important than sometimes believed because there had been a mass evacuation of the Broad Street area before the pump handle was actually removed. In any case, the leading public health authorities of Britain, in recognition of Snow's contribution, have now dedicated beside the site of the pump a London pub which is to be known to posterity as "The John Snow." The dedication of this Broadwick pub took place on 5 July with Professor Bradford Hill once again giving the principal address. John Snow relics are to be permanently on display as an attraction to historically-minded medical students who may wish to drop in for a lager.

Although it may seem slightly surprising to have a public house dedicated to a teetotaler, one can readily agree with the Editor of *The Medical Officer* who points out that

English pub names are so redolent of the national history that this renaming of a London house in the centenary year after so worthy a medical pioneer seems to us to be entirely appropriate, and perhaps especially so if we recall the part which brewery workers played as "controls" in Snow's investigation of the Broad Street cholera. Hitherto the great names in preventive and social medicine have been mainly commemorated in teaching institutions and health centres, but whilst the latter are in such short supply we hope that Messrs. Watney, Combe and Reid's example may commend itself to the owners of other inns in places associated with famous public health workers.

I am indebted to Sir Allen Daley for drawing my attention to these articles. Although his name is not mentioned in the accounts one suspects that the original idea was his and that Professors Mackintosh and Hill, long known as students and admirers of John Snow, readily co-operated.

J. F. F.

Dr. Corner Elected Member of Royal Society

Dr. George W. Corner, Director Emeritus of the Department of Embryology, Carnegie Institution of Washington, Baltimore, has been elected a Foreign Member of the Royal Society of London. Dr. Corner is currently serving as President of the American Association of the History of Medicine (1954-56), and he has just been appointed by Dr. Bronk to a new post as Historian of the Rockefeller Institute in New York; this will involve preparing an official history of the first fifty years of the Institute. In this work he is to be assisted by Dr. John B. Blake, a member of the Department of the History of Medicine at Yale.

Dr. Alfred M. Hellman (1880-1955)

The current issue (vol. 8, *1955*, no. 1) of *The Academy Bookman*, published by the Friends of the Rare Book Room of the New York Academy of Medicine, is dedicated to the memory of Dr. Alfred M. Hellman, whose activities in gynecology, obstetrics, and the world of books were outstanding through so many years. Medical historians will recall in particular his *Collection of Early Obstetrical Books* (1952), partially reprinted from the *Yale Journal of Biology and Medicine* (24, *1951*, 210-219) and issued with the assistance of his daughter, Dr. C. Doris Hellman Pepper, an historian and bibliophile in her own right. The 39 works minutely described in this catalogue, published from 1513 to 1635, constitute a rare assemblage for the history of obstetrics during that period.

An Anniversary in Orthopedics

On 30 September 1955, the Nebraska Orthopedic Hospital at Lincoln celebrated its fiftieth anniversary. In so doing it commemorated also the outstanding contribution of Dr. H. Winnett Orr, who received the first patients there in 1905 and whose services, except for two years of military duty abroad in World War I, have been continuous since.

Coins for Use by Patients with Hansen's Disease

The unholy fear of contact with patients suffering from Hansen's Disease (leprosy) was extended even to coins which these unfortunate patients had to use. Several examples of this prejudice are in the writer's possession. One of these coins (Fig. 1) was obtained from Dr. Ezra Hurwitz, who has been in charge of the Palo Seco Leper Colony in the Canal Zone for many years. Under difficult circumstances he has done marvelous work. The history of the development of coinage for this leprosarium is given by Mrs. Hurwitz in a letter of 25 January 1955, which is also in the writer's possession:

> When General Gorgas first undertook the sanitation of the Panama Canal in 1904, there were a few families who were living as outcasts on the Marañon beach in Panama City because there were a few cases of Leprosy amongst them.
>
> General Gorgas gathered these cases of Leprosy and took care of them at the Empire Hospital until the colony at Palo Seco was ready to receive them in 1907.
>
> About 1919 with the introduction of chaulmoogra oil in the treatment of Leprosy there were considerable improvements made in the Colony, and [it] was then decided to give patients as much opportunity [as possible] to earn money doing the work necessary in the Colony and [by] giving them plots of land for cultivation and purchasing the produce for use in the Colony.
>
> Among the other improvements made at that time coins were issued for the patients, to be used in the Colony only because the idea of money that had been handled by patients with Leprosy afterward being handled by other persons outside the Institution was repugnant, and at that time it was thought that the disease could possibly be transmitted by such contact. The coins were issued in the same denominations as U. S. currency.
>
> The money was made in the following denominations: 1 to five cents, in brass with a square hole; ten cents, twenty-five, fifty, and one dollar, in aluminum and a round hole. . . . It was used in the Colony until recently.

Another example of coinage for such a colony was the money used at Culion in the Philippines (Fig. 2), a specimen of which is listed in Horatio Robinson Storer, *Medicina in Nummis* (1931), no. 4366. Here the values were in centavos and pesos. It is important to note that both colonies used the coins until recently. It is also interesting that in both cases it was the United States Government which issued the coinage. That at least one other nation had similar coinage is clear from Storer's no. 5119, from the Leper Hospital at Maracaibo, Venezuela, where the values were in bolivars. The matter is of interest not only to the writer but also to Stanley Stein, editor of *The Star,* which is published by patients of the U. S. Public Health Service Hospital at Carville, Louisiana. Further information on such coinage is invited.

JOHN GOLDMAN

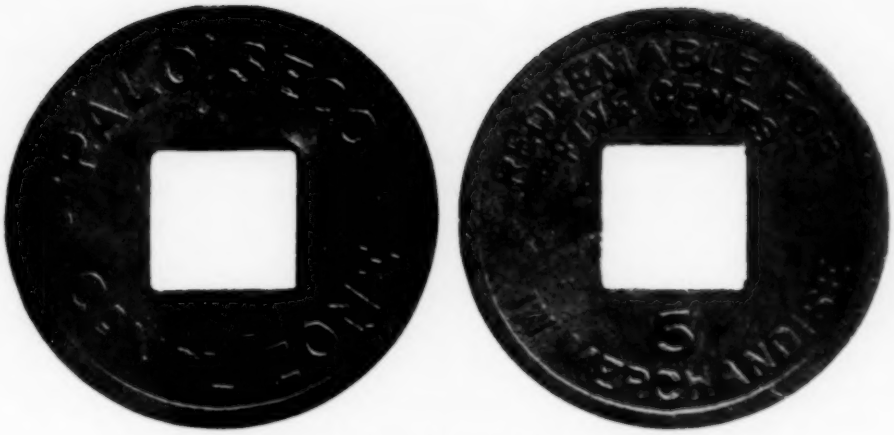

Fig. 1. Palo Seco, Canal Zone. 5 cents.

Fig. 2. Philippine Health Service. 1 peso.

Six Skulls in Search of a Tomb

Under this headline, cleverly adapted from the title of the famous play by Luigi Pirandello, *Il Messaggero di Roma* for 5 June 1955, related a story which has been reported also in America in the dispatches of Arnaldo Cortesi to *The New York Times* and in that charming sheet of the Cultural Division of the Italian Embassy, *The Italian Scene.*

In 1945 the physiologist and pathologist Gaetano Pieraccini became, at the age of eighty, mayor of the city of Florence. Thirty years before he had commenced his study of the hereditary characteristics, over three centuries, of ten generations of the Medici family, and in particular of the ravages inflicted by gout and kindred ills upon successive members of that famous clan. Sufficient evidence of the serious purpose of Prof. Pieraccini's work is his four-volume publication *La stirpe de' Medici di Cafaggiolo: Saggio di ricerche sulla trasmissione ereditaria dei caratteri biologici* (2d ed., Florence, Vallecchi, 1947). The opportunity to study and photograph the skeletons was irresistible to him even at the age of eighty, and with his new authority he ordered that the tombs in San Lorenzo be opened and the bones disinterred. The fact that Michelangelo's glorious statues had been removed earlier from the dangers of war and were still absent from Florence facilitated the opening.

The last exhumations were made in 1949. The bones were studied, tested, measured, labeled, photographed, and were eventually returned to their resting-places with the exception of the craniums. These were ranged in a neat row on a shelf in the Anthropological Institute of the University of Florence. The hue and cry from Florentines, lay and ecclesiastical, was immediate, continuous, and loud. It went on for some years while Professor Pieraccini and his co-workers pleaded in vain for a dry, chemically-treated "Cranioteca" where preservation would be better and where, a hundred years later, pathologists could still examine the skulls.

The Ministry of Public Instruction in Rome has now reiterated its support of the protesting voices, and the craniums will be returned to their original resting-places, where some had lain peacefully for five centuries. The work was in fact begun on 26 July. In truth the skulls number twenty-three rather than six. But the greatest concern has been about the six from tombs adorned with the artistry of Verrocchio and Michelangelo. This was indeed one plea of the pathologists, that such masterpieces should not again be disturbed. But doubtless by now *Night* and *Day, Twilight* and *Dawn* will once more have dangled briefly in mid-air, suspended by a cable, while the skulls of Giuliano and of Lorenzo II rejoined their bones. Meanwhile Queens Catherine and Marie of France have slumbered undisturbed by pathologists at Saint Denis, Popes Leo X and Clement VII in Santa Maria sopra Minerva in Rome.

Latin "UTERUS"

An etymological question is raised in a letter to the Editor of *The Lancet,* 6 August 1955, by C. W. F. Burnett of West Middlesex Hospital, Isleworth. In Latin the word *uterus* can mean either belly or womb. It is sometimes thought to be derived from Greek ὑστέρα, but a connection with *uter,* meaning a bag or bottle made of an animal's hide, has also been suggested. On a recent visit to the Cenral Sudan Dr. Burnett watched sacks of gum being unloaded from camel trains. The sacks were made of goatskin, and he points out in his letter and by a photograph the remarkable resemblance they bore in shape to the uterus. They had indeed a narrow neck, a triangular cavity, two cornua, and a convexity corresponding to the fundus. On the basis of this evidence he would therefore derive *uterus* from *uter.*

Edgar Fahs Smith Memorial Collection in the History of Chemistry

Dr. Charles W. David, Director of Libraries at the University of Pennsylvania, announces that the Smith Collection has been moved to Room 420 of the Hare Building, 36th and Spruce Streets, Philadelphia 4, Pa., and that it has been reopened for service. As of July 1, a half-time custodian has been appointed. Miss Eva V. Armstrong, the Corresponding Secretary, will also be in the Smith Library on Wednesdays between the hours of 11 a.m. and 3 p.m. Communications should now be addressed to the Edgar Fahs Smith Collection at the address given above. Dr. Claude K. Deischer has been appointed Acting Curator (address: John Harrison Laboratory of Chemistry, 34th and Spruce Streets, Philadelphia 4, Pa.).

Osler, Arms and the Man

A recent biographer of Welch is ignorant enough of the early Hopkins spirit to write that Osler was "afraid of Mall" and to quote an (apocryphal?) early prognostication by Kelly that Osler's ambition to get to London and be knighted would cut short his stay in Baltimore! Perhaps one of Kelly's more ponderous jests? Certain it is that Osler stayed 15 years and would have remained another five years before retiring to Montreal if we North Americans would have let him do it, as he said, "above ground." And the truth is that when at 55 he accepted the comparative leisure of Oxford, he did not accept the baronetcy which practically went with his post as Regius Professor. Like Gallio he cared for none of these things, but when, as is most unusual, it was offered him again six years later in 1911, he felt it would be ungracious to decline. What amused him more than it did Lady Osler who had just recently raised the servants' wages, was that they had to be raised again. In those halcyon days there were real servants and they rose in the social scale with "the master." In 1905 he had also dodged the offer of

a court appointment, doubtless with a graceful excuse to the authorities, but the one he gave his wife was *dis*-gracefully to the effect that he did not want to be at the mercy of every little royal belly-ache.

A baronet's coat of arms must be above suspicion, and the Heralds' College would not accept Fido, the dog labelled FIDELIS on the family silver, for it could not be proved that he had not been introduced a few generations back by a mere female or an auctioneer. So Osler chose as his crest the beaver and fleur-de-lis for Canada, and for the arms—to paraphrase the heraldic jargon—three wavy bands, white and blue, and three pilchards, fish-colored and rising for air ("haurient"), with his own and Marcus Aurelius's motto AEQUANIMITAS, such arms to be borne also by all the descendants of his father. One among several who regretted Fido complained that "a dead fish seems to lack something." Now the best pilchards, like the best Oslers, came from Cornwall where the family had been seafaring folk, though not professional fishermen but small shipowners and privateersmen. When, however, I asked W. O. why the fish, he gave me a characteristic and probably true answer: so that his son Revere, whose only passion at 15 was fishing, might feel at home with his honors when they should devolve on him. The lad was horrified at the prospect of inheriting his father's "Sir" without his genius. Alas, he needn't have worried. Fate let him live only long enough to give promise of an interesting blend of his father's literary and Paul Revere's artistic talents.

Since Lady Osler's death in 1928 the house which the early Rhodes Scholars nicknamed The Open Arms has been a neglected adjunct of the Regius professorship, serving as a students' hostel, so it is good news that at last a new "Regius" is going to occupy part of it; also that, thanks to the Osler Club of London, a regulation "lived-here" plaque is to be affixed to its wall. Let into the plaque is a small tile bearing the motto and the arms in their proper colors. It is made by a famous pottery, measures about 5 by 3 inches and can serve as a bright and practical paper-weight. If there is a demand for them the pottery is prepared to reproduce these tiles for about the equivalent of nine dollars in England. Customs charges, etc., will raise the cost, delivered in the United States, to perhaps seventeen dollars. Osler fans who feel they must have one may "bespeak" it by notifying (not, please, by paying) me at the Osler Library.

W. W. FRANCIS

Centennial of Carlos Finlay's Graduation from Jefferson Medical College

The centennial of Carlos Finlay's graduation from the Jefferson Medical College, Philadelphia, was commemorated by the College in a series of meetings, 22-23 September 1955. A symposium on yellow fever, under the chairmanship of Dr. Fred L. Soper, occupied the morning and afternoon of the first day. Enlisted as speakers were Dr. Pedro Nogueira (Cuba), Dr. A. F. Mahaffy (Canada), Dr. John Bugher (U.S.A.), Dr. J. Austin Kerr

(India), Dr. Octavio Pinto Severo (Brazil), Dr. Jorge Boshell Manrique (Colombia), Dr. Wilbur Downs (Trinidad, B.W.I.), and Dr. Soper.

In the evening, a biographical symposium was held, in a hall named for S. Weir Mitchell, at the College of Physicians of Philadelphia, which had elected Finlay to its associate fellowship in 1903. Speakers at this session were Dr. Horacio Abascal (Cuba), Dr. Leandro M. Tocantins (U.S.A.), and Dr. Philip S. Hench (U.S.A.). Dr. Tocantins, the secretary of the College of Physicians, recalled, chiefly by means of unpublished letters, what he termed the intellectual partnership between Finlay and S. Weir Mitchell, who was Finlay's preceptor at Jefferson and subsequently a firm friend and supporter.

On the second day, addresses were delivered by Dr. Felix Hurtado, the Cuban ambassador, Dr. Joseph E. Smadel (U.S.A.), and Major-General Alexander J. Orenstein (Union of South Africa). There were, further, a conferral of honorary degrees, and the unveiling, by Alfredo Finlay, of a sculptured portrait of his grandfather. While the major part of the proceedings was appropriately historical in orientation, due attention was paid to the fact that the final eradication of yellow fever is, as Dr. Soper described it, still unfinished business.

W. B. McD., 2D

L'Istituto e Museo di Storia della Scienza di Firenze

It may interest our readers to know that the important Museum of the History of Science at Florence, which was conceived by Dr. Andrea Corsini and outlined in a paper delivered at the 2d Congress of the History of Medical and Natural Sciences in Bologna in 1922, is in a flourishing state. Dr. Corsini's suggestion, which was eventually published in *Archivio di Storia della Scienza* ("Urgenza di assicurare il patrimonio storico-scientifico italiano e su i mezzi di provvedervi," 1924, 5), was enthusiastically accepted, and soon thereafter in a room of the Faculty of Medicine of Florence on the Via degli Alfani, the Museum had its origin, starting at first with a small nucleus of material relating to the history of science. Dr. Corsini's realization of the great value of having all the rich patrimony of the history of science in Italy brought together in Florence subsequently led to the First National Exposition of the History of Science to which contributions were sent from all over Italy. This wealth of material made additional space imperative, and the Museum was moved in 1929 to its present location in the Palazzo Castellani on the Piazza dei Giudici, near the Uffizi Gallery. The collection continued to grow rapidly as gifts and items from other institutions were deposited in the Museum which now possesses not only the priceless Galileo material, but also the various scientific instruments from the Accademia del Cimento. The instruments are well grouped into optical, mathematical, astronomical, cosmographic, mechanical, medical, biological, chemical, and technical categories.

The present quarters are already inadequate, and it is hoped that as the collections grow more funds may be found to help meet the increased responsibilities of the Museum which has become such an important depository of the heritage of the history of science.

The venerable Professor Corsini, who celebrated his 80th birthday in 1954, is still very much the energetic Director of the Museum. He is ably assisted by Dr. Maria Luisa Bonelli who teaches students from the University in the Museum. She and Dr. Corsini have also just issued a detailed and well-illustrated catalogue of the Museum published in 1954 under the title *Catalogo degli strumenti del Museo di Storia della Scienza* (viii, 394 pp., 16 full page plates and many other illustrations).

Museo Nazionale della Scienza e della Tecnica "Leonardo da Vinci"

As noted in this journal some two and a half years ago (Vol. VII, pp. 217-219), the imposing Museum of Science and Technology "Leonardo da Vinci" at Milan was then under reconstruction at Via San Vittore 19 as a venture sponsored jointly by the Italian Government and the philanthropic director, Signor Guido Ucelli di Nemi. During the interval Signor Ucelli has had the pleasure of seeing the plan which he had originally envisioned come into being in a manner that should bring him, the Italian people, and all others interested in the history of science great satisfaction. The Museum successfully presents the growth of ideas from their conception to their practical application in fields such as transportation and communication and follows them from ancient times to the jet age.

The central feature of the museum, as Professor Castiglioni explained in his note aforementioned, is the Hall of Leonardo da Vinci in which constructions have been made (from his notebooks) of all of his various "engines" of defense, offense, and transportation, including his plan for a man-propelled aeroplane and also his helicopter. The walls of Leonardo Hall are adorned with sheets from his notebooks (some original, others reproduced) and some little-known relics of his handiwork.

Parallel to this gallery is another illustrating the history of physics and another, the history of chemistry. In these latter exhibits, Italian discoveries have been stressed, but not to the exclusion of contributions, such as those of Lavoisier, from other countries. In a large hangar outside the main museum building are airplane models of every description, beginning with Leonardo and including models of the Wright brothers' plane, the Spirit of St. Louis, the earliest dirigible, and the first jet-type aircraft (Italian 1941). Beside the hangar is a large plot of land on which an addition is to be erected to extend the premises of the present Museum.

In planning the reconstruction of the building, space was allocated for amphitheatres and meeting rooms with the thought of attracting congresses and special meetings, and during a visit in September of 1955 it was gratifying to see two congresses in progress. The Museum was formally opened on 15 February 1953. An advisory committee to plan for future development has

been formed with a large membership representing all phases of science, technology, and the history of science. They held their first meeting in June 1955, following which a brochure was issued under the title: *Prima Riunione del Comitato Permanente per lo Sviluppo del Museo.* A handsomely illustrated monograph by Ferdinando Reggiori has also been recently issued (*Il Monastero Olivetano di San Vittore al Corpo in Milano*), describing the monastery and its two beautiful cloisters which were destroyed during World War II and have now been reconstructed according to the original plan.

J. F. F.

Eighth International Congress for the History of Science Florence, 3-9 September 1956

The Eighth International Congress for the History of Science will convene in Florence between the 3d and 9th of September 1956, concluding in Milan.

The committee in charge of organizing the congress is composed of Prof. V. Ronchi, President; Prof. G. Abetti, Prof. M. Atazzi Mancini, Prof. R. Almagià, Prof. N. Beccari, Prof. B. Borghi, Prof. L. Campedelli, Prof. G. Carobbi, Prof. U. Cassina, Prof. A. Corsini, Prof. A. Frajese, Prince Ginori-Conti Dr. G., Avv. M. Gobbo, Prof. P. Lamanna, Prof. A. Sestini, Ing. G. Ucelli, Gr. Uff. Vannini Parenti, Members; Dott.ssa Maria Luisa Bonelli and Prof.ssa M. Timpanaro Cardini, Secretaries.

The congress will be divided into four sections: (i) History of mathematics, physics, and astronomy, (ii) History of chemistry and pharmacy, (iii) History of medicine and biology, and (iv) History of applied science and technology. Members who wish to present one or more communications to the congress are requested to send their papers before 1 April 1956. No papers will be accepted after this date. The papers should not exceed five typewritten pages and should be accompanied by a summary in English, French, and Italian of no more than 280 words.

Letters of enquiry about the congress should be addressed to the Office of the Secretary, Museo di Storia della Scienza, Piazza dei Giudici 1, Firenze. The registration fee of $10 for each member and $5 for accompanying relatives may be addressed directly to the agencies of Wagons-lits Cook or Thos. Cook and Son, to which application must also be made for hotel accommodations.

American Association of the History of Medicine, 19-21 April 1956

The twenty-ninth annual meeting of the Association will be held at Durham, North Carolina, on April 19, 20, and 21. Dr. George Rosen, Chairman of the Department of Health Education at Columbia University, 600 West 168th Street, New York 32, will serve as Chairman of the Committee on Program Papers and Lectures.

Book Reviews

RALPH H. MAJOR. *A History of Medicine.* Springfield, Illinois, Charles C Thomas, 1954. 2 vols., 1155 pp., illus. $18.50.

Reviewed by HENRY R. VIETS, *Acting Director, Boston Medical Library, Boston, Massachusetts.*

The writing of a new history covering the period from the dawn of the development of the art and science of medicine to the twentieth century is a formidable task. As Dr. Major in his own Preface says, "it takes courage to project still another history of medicine." Only a man of formidable determination, who has powers of sifting and winnowing the vast accumulation of knowledge in regard to our specialty, would have the fitness to accomplish such a task. The author of these two volumes—widely read, erudite, discriminatory, and particularly qualified because of his extensive travels—is such a man. In his monograph, a continuous account, punctuated by the names of eminent physicians and with notes of a brief biographical and bibliographical character supplementing each section of the work and with a large number of carefuly chosen ilustrations, he has produced an important work which will have its place in the history of medicine itself.

The sections of the two volumes are arranged in their usual chronological sequence, divided, with their approximate page coverage, as follows: Dawn of Medicine (100); Greek Medicine (60); Roman Medicine (60); Middle Ages (135); Renaissance (120); Seventeenth Century (85); Eighteenth Century (75); Nineteenth Century (280), including 60 pages on the Rise of American Medicine; Twentieth Century (135). It will thus be seen that there is a more extended analysis of the medicine of the Middle Ages and the Renaissance (255 pages) than of the whole seventeenth and eighteenth centuries (160 pages), possibly reflecting some of the chief interests of the author. The nineteenth century is even more fully covered, with 280 pages devoted to it, and with 135 pages given to the twentieth century; the total for the last 150 years thus amounts to 415 pages, or more than one-third of the entire book. This is a normal proportion and only the 120 pages devoted to Greek and Roman medicine and particularly the 160 for the 200 years between 1600 and 1800 appear a little scanty. The coverage, however, is remarkably complete and only a few major names are missing. Surely James Thacher ought to be included if only for his *American Medical Biography.*

The book was designed particularly for medical students and practitioners. It is eminently readable and should find a place on the shelf of every doctor who honors the past. Like many books of its type, however, it will be chiefly used for reference, for the correct spelling of a man's name, the dates when he lived and his important published contributions are data

continuously sought for by many physicians and medical librarians. One is likely to look up a name rather than a subject and it would have been more helpful to have had the index of names published at the end of the book following that for subjects. As the book is only a little over 1,000 pages it would have been more easily used had it been published in one volume. Its use in medical libraries for reference will be extensive, for the author has been extremely accurate in giving dates, names, and places, and only a few minor typographical errors were noted. The covers, in rough cloth, leave much to be desired, for they will soon become dirty and badly worn. A smooth buckram would have been much better and all librarians would have been appreciative. In spite of these minor defects the book provides an excellent brief history of medicine and should prove most useful to those for whom it was designed.

LUDWIG H. HEYDENREICH. *Leonardo da Vinci.* London, George Allen and Unwin, Ltd.; New York, The Macmillan Company; Basel, Holbein Verlag. Authorized translation from German, 1954. 242 half-tone figs., 210 pp. xxi pp. (Notes).

Reviewed by the EDITOR.

This exhaustive bibliographical study may properly be looked upon as a definitive biography of Leonardo which is not likely soon to be superseded. The author, Ludwig H. Heydenreich, has been a student of Leonardo for many years, as his many books and papers on the great Florentine testify. Dr. Heydenreich, a graduate of Hamburg and a one-time Lecturer on the History of Art at Berlin, was formerly (1943) Director of the Institute of the History of Art at Florence, and more recently (1947) Director of the Central Institute of Art History at Munich, a position which he now occupies. The volume was published in the Netherlands. The translation, which is in every way excellent, was made jointly by Mrs. Dora Jane Janson, New Rochelle, New York, Mr. Malcolm Barnes, London, Mr. Glyn T. Hughes, Basle, Mr. James Chesterman, London, and Frau Marlene Kesser, Basle.

The author states in his "Foreword" that anyone who studies Leonardo's vast range of interests must be aided by some guiding principle, and he states that his guiding principle in writing this book "has been to show the interrelationship between Leonardo's artistic and scientific work." He believes that Leonardo's science cannot be fully understood without an appreciation of his artistic endeavor and that the two are so closely intertwined that to study one without the other would be a fruitless undertaking.

Leonardo's life is outlined in some detail and it includes much new material, especially that brought to light on the occasion of the 500th anniversary of his birth which was celebrated in all parts of the world three years ago. The account of Leonardo's painting begins with the well-known

anecdote about the two angels which Leonardo had a hand in adding to Verrocchio's *Baptism of Christ*. The author states that the most exquisite of the two angelic figures, namely, that on the left, is definitely by Leonardo and "is his work entirely." From this we must infer that the other angel was done by the hand of his gifted pupil, Raphael, who in the past has sometimes been given credit for both figures on the authority of Vasari. The author believes that Verrocchio reveals in this canvas that he had become conscious of the new forces that were at work in the world, especially in the world of painting, and that it marks a turning point in the history of modern art. Leonardo's other pictures are described in some detail, including the two Madonnas of the Rocks and concluding with the Mona Lisa which "more than any other picture has come to represent Leonardo's mind and work for all subsequent generations."

There is a forceful account of Leonardo as a sculptor. Later the author considers his excursion into architecture, engineering, mathematics, optics, and mechanics, but the chapters which will be of the greatest interest to readers of this Journal are those on anatomy and physiology and zoölogy and botany. Much has been written concerning Leonardo's physiology and his unusual capacity for turning anatomical facts into physiological language. In no area is this more brilliantly demonstrated than in his studies on the vascular system in which he describes the heart and its valves and reveals his appreciation of the fact that they were so disposed as to prevent reflux of blood toward the left heart after it had entered the aorta; he was also familiar with the valves in the veins which prevented the reflux of blood toward the tissues. The late Arnold C. Klebs, who had spent much time on the Leonardo plates, concluded that Leonardo was undoubtedly familiar both with the lesser and the general circulation and that he thought the idea so obvious that he failed to record it as a new discovery.

In his excursions into zoölogy and botany, Leonardo concerned himself, among other things, with fossils, and especially those which are found in high places and yield evidence of shells and other flora and fauna of the seas—evidence which led him to conclude that even the foothills of the Alps must one day have been under water. Everything interested him, and he recorded everything in his precious notebooks, only a few of which have actually survived.

A. C. Crombie. *Oxford's Contribution to the Origins of Modern Science.* Oxford, Basil Blackwell, 1954. 27 pp. 2s. 6d.

Reviewed by William B. Walker, *Graduate Fellow, Department of the History of Medicine, Yale University School of Medicine.*

In this brief paper, which was read at the meeting of the British Association for the Advancement of Science at Oxford last September, Dr. Crombie devotes himself principally to two periods, the later Middle Ages and the

seventeenth century, which he characterizes as times . . . of especial brilliance during which modern science took shape in the West. . . ."

The earlier outburst of activity occurred shortly after the founding of the University and is primarily associated with the name of Robert Grosseteste and his successors, notably Roger Bacon and William of Ockham. Two lines of inquiry were developed, that relating to general scientific method, and the more specific work on dynamics pursued by the fellows of Merton College.

After an interval of scientific decline, which Dr. Crombie does not attempt to explain, a gradual revival begun in the Elizabethan period culminated in the proliferation of scientific activity at the time of the English Civil War. Particular attention was given to anatomy, physiology, and chemistry, and the familiar story of the creation of the Royal Society is an outgrowth of this.

Dr. Crombie's summary, while at times discursive and overly crowded with detail, does convey a sense of the rôle of Oxford in promoting the development of modern science. This reviewer feels the scientific decline of the late fourteenth and fifteenth centuries may have been owing in part to the ravages of the Black Death and the internecine struggles of the Wars of the Roses. He would also be less inclined to credit the abilities of John of Gaddesden and his medieval colleagues, while reserving more praise for the seventeenth century scientist John Mayow. Nor does he share Dr. Crombie's concern that the historian of science will be unduly influenced by an ". . . enchantment . . . of the historical past. . . ." On the contrary, he feels that it is the objectivity prevailing in present-day historiography which will, in Dr. Crombie's own words, ". . . improve the discipline and order of our understanding of the scientific aspect of our civilization. . . ."

JOSEPH NEEDHAM. *Science and Civilization in China.* Vol. I, *Introductory Orientations.* With the research assistance of Wang Ling. New York, Cambridge University Press, 1954. xxxviii, 318 pp., illus. $10.00.

Reviewed by ILZA VEITH, *Assistant Professor in the History of Medicine, The University of Chicago, Illinois.*

THIS introductory volume to a seven-volume work is indeed extremely promising. For the first time an attempt has been made towards a comprehensive and well-integrated study of Chinese science and its history which even in their various components have never been fully analyzed. Dr. Needham is uniquely prepared for his ambitious undertaking: as an eminent biochemist he is a Fellow of the Royal Society and Sir William Dunn Reader in Biochemistry at the University of Cambridge; as a historian of science he has published a standard work on the history of embryology; and as a sinologist he has the distinction of being a Foreign Member of the Academia Sinica. In addition, he has traveled widely in China, and his

knowledge of the country, its people, and language is intimate and thorough.

In their specific setting of the scene Dr. Needham's introductory orientations are as useful to the specialist as they are "to all educated people," for whom this book is intended "whether themselves scientists or not . . . who are interested in the general history of civilization and especially the comparative development of Europe and Asia." The geographical and historical introductions and the characteristics of the Chinese language present an excellent background for the conditions of travel of scientific ideas and techniques between China and Europe which are described in considerable detail. The magnificent illustrations and the historical and linguistic tables enhance the beauty of the book and add much to its clarity.

It is interesting that of their many scientific endeavors the Chinese considered only astronomy and meteorology worthy of inclusion in their detailed official histories. Descriptions of other achievements and technological advances were relegated to separate treatises on the subject, or found mention in many different miscellaneous collections, memoirs, or "pen conversations." While this diversification of sources must have aggravated Dr. Needham's search for material, he was, on the other hand, spared much of the labors of critical analysis, since it could be assumed that these "texts have never been intentionally interfered with, partly because the Confucian scholars considered them too unimportant, and partly because until modern times it would never have occurred to any Chinese scholar that the slightest interest attached to placing a piece of scientific knowledge or a technical process earlier than its proper place." This statement does, of course, not apply to Chinese medical writings where cases of "priorism" are not infrequent.

Although this first volume is highly informative, it cannot be considered an entity by itself and therefore critical analysis concerning omissions or interpretation must await the publication of the entire work. One minor point of criticism valid now is the system of transliteration, or romanization, chosen by Dr. Needham. Admittedly, few if any of the traditional systems convey the true sound of Chinese, but some of them, particularly the Wade system, which is the basis of most Chinese-English dictionaries and history texts have become generally accepted. Dr. Needham's choice of "h" in place of the conventional apostrophe (Thang and Chhing dynasties instead of T'ang and Ch'ing) hardly helps to reproduce the Chinese sound, but may confuse those readers who are not very familiar with Chinese names and history.

The subsequent volumes now in manuscript will deal with the history of scientific thought, mathematics, and the sciences of the heavens and the earth, physics, engineering, and technology; chemistry and industrial chemistry; biology, agriculture, and medicine; and, finally, the social background. The publication of these volumes is eagerly anticipated.

OTHO T. BEALL, JR. and RICHARD H. SHRYOCK. *Cotton Mather. First Significant Figure in American Medicine.* Baltimore, Johns Hopkins Press, 1954. ix, 241 pp. $4.00.

Reviewed by GEORGE ROSEN, *Professor and Head of the Department of Public Health Education, Columbia University, New York, New York.*

THE Puritan has had his ups and downs in American historiography. Reared by the early, filiopietistic American historians, he fell into disfavor under more critical examination. Within recent decades a revisionist tendency has marked the study of Puritanism and the Puritan tradition. This trend has had some interesting manifestations of which the volume under review is one.

Long a symbol of Puritan theocracy in Colonial America, Cotton Mather has recently been "rediscovered," with the result that a more balanced picture of the man, his thought, and his achievement is beginning to take shape. Beall and Shryock contribute to this better understanding by examining his medical thought and his contribution to medical practice. From an investigation of Mather's writings, non-medical and medical, as well as from his part in the Boston inoculation episode of 1721-22, they have concluded that he was "the first significant figure in American medicine." Furthermore, the authors believe that Mather "provides an almost unique mass of evidence concerning early American medicine, with all its implications for the social and cultural history of the times."

The evidence from which these opinions chiefly derive is contained in Mather's unpublished work *The Angel of Bethesda,* and the authors deserve commendation for having included copious selections from it. Indeed, these comprise almost half the book, and enable the reader to form his own judgment on the conclusions reached by Beall and Shryock. As a framework for this material, other chapters consider Mather's approach to medicine, the medical background of the seventeenth and early eighteenth centuries against which he wrote and acted, and the medical materials and themes in his unpublished works.

Mather's earliest scientific interests were directed to medicine, and this preoccupation with medical theory and practice persisted throughout his life. He was well-read in the literature of medicine, and medical items, comments, and themes occur in such writings as the *Magnalia, The Christian Philosopher,* the "Curiosa Americana" and a variety of sermons. Mather's approach to medicine was religious as well as scientific and he drew no sharp line between the two. Thus, he held the doctrine that disease came into the world through original sin, and at the same time he endeavored to acquire medical knowledge so that he might help people. Out of these dual streams of thought he developed a medico-theological synthesis which came to fruition in *The Angel of Bethesda.*

Stress is placed by the authors on Mather's concern with the medical care of the poor, and the influence in this direction of early German

pietism. At the same time one ought not to overlook the fact that the English clergy had provided home remedies and medical care to the poor long before Mather's day. In fact, in dealing with the medical care problems of their own day, Samuel Hartlib (1641) and Gerrard Winstanley (1652) proposed that the clergy engage to a greater extent in the provision of medical care to the poor. Hence the relation of American circumstance and action to those of the English was probably not one of parallelism but rather of direct filiation. In short, Mather's integration of theology and medicine in the practical as well as the theoretical sphere is in line with an English tradition of an earlier period as well as of his own time. Furthermore, he was aware of endeavors on the Continent by physicians motivated by religious ideals to provide medical guidance, advice, and prescriptions to the poor. For example, Mather in the chapter of the *Angel* on scurvy quotes with approbation from the *Poor Mans Physician* by Monsr. D'Ube. This source is *Le Médecin des Pauvres, qui enseigne le moyen de guérir les maladies par des remèdes faciles à trouver dans le Païs, & preparer à peu de frais par toutes sortes de personnes* by M. Dubé, doctor of medicine. This book was published in its fifth edition in 1678.

Hitherto, Mather's main claim to fame in the medical field has been his rôle in the introduction of inoculation for the smallpox during the Boston epidemic of 1721-22. Beall and Shryock now point out that this action was motivated not only by a desire to apply knowledge to the welfare of mankind, in this instance to improving the health of the community by combatting an epidemic, but also by a coherent animalcular theory of disease causation. This appears to have some plausibility on the basis of the evidence presented. There is no doubt that Mather was widely read, and he knew the theory of animate contagion as expressed in the writings of Athanasius Kircher, Benjamin Marten, Hartsoeker, and others. However, the authors seem to imply that he had developed his theory of animalcular causation of communicable disease before the smallpox epidemic, and that his activities in 1721 were a consequence. On this point the evidence does not seem to bear out this theory. In fact, quite the reverse seems to be the case. Mather throughout emphasized that he first learned of variolation from a Negro servant. In the *Angel* he says: "I was first instructed in it, by a Guramantee-servant of my own, long before I knew that any Europeans or Asiaticks had the least acquaintance with it, and some years before I was enriched with the communications of the learned, whose accounts I found agreeing with what I received of my servant. . . ." Apparently, then, Mather learned of the practice before 1713. Thus it would have to be shown that he had developed his theory of animalcular contagion prior to this date and that it led him to propose variolation. For this sequence there is no evidence. Mather relied to a very large extent on Marten's book, but this was not published until 1720 and he had already in 1716 written to Dr. John Woodward proposing that variolation be introduced as a general pre-

ventive measure. It seems plausible to suggest, therefore, that the theory of animate contagion simply supplied a theoretical rationale for an empirical procedure already accepted. Furthermore, as the *Angel of Bethesda* was never published and therefore exerted no influence that is detectable on the subsequent cause of medicine in America or Europe, Mather's contribution to medicine is the introduction of variolation in Boston. There is no doubt that this contribution alone assures him a permanent place in the history of medicine.

This book is a valuable contribution to our knowledge of Cotton Mather and thus to our understanding of New England culture at a point of transition. By making available large sections of *The Angel of Bethesda,* Beall and Shryock have performed an important service for all students of American medical history and culture. This book is highly recommended to all interested in the mind of colonial New England and in the history of American medicine.

MAURICE DAVIDSON and F. G. ROUVRAY. *The Brompton Hospital: The Story of a Great Adventure.* London, Lloyd-Luke, 1954. 152 pp., illus. 21s.

Reviewed by P. D'ARCY HART, *Director, Tuberculosis Research Unit, Medical Research Council, Great Britain.*

A CENTURY ago, a special hospital was founded by Sir Philip Rose and his friends to deal with the situation where "consumption, the most frequent and fatal disease in existence, which, according to a high authority, destroys one third of our entire population, finds the doors of all our hospitals closed against it." In this elegant volume Dr. Davidson and Mr. Rouvray, long associated with the hospital as physician and house governor respectively, trace its history from the charitable "Hospital for Consumption and Diseases of the Chest," possessing 20 beds, through its long career as voluntary hospital in which it has become the leading institution in the British Commonwealth for the treatment of chest disease, until 1948 when, as the 350-bedded Brompton Hospital, it was incorporated into the new National Health Service.

Intimate details are given of the enthusiastic management and the day-to-day events in a hospital of the Victorian era, of the part played by society patronage, and of the many famous professional men serving on the staff—famous, be it noted, also in the most varied branches of medicine. Thus, among the earlier medical staff were Robert Liston, FRS, surgeon and anatomist; John Hutchinson, physician, statistician, and research worker in the prevention of accidents in coal mines; Richard Quain, FRS, physician and author of "Quain's Anatomy"; James Burdon Sanderson, FRS, physician and physiologist; and, perhaps greatest of all, Lord Lister, FRS. The more recent history is equally informative, and a procession of distinguished physicians and surgeons passes before us.

The story will be of interest to all students of hospital history, though many will regret its failure to reflect the social changes of the century in the world outside, and the little reference to the relation of this famous treatment centre to the expanding tuberculosis services and, in particular, to those provided by the local authorities. Moreover, the outstanding contribution of the hospital to medical and surgical techniques is only lightly touched upon. Perhaps this emphasis is inevitable in a book designed for the lay reader, who may perhaps feel that there is over-exaltation of the *hospital* almost as an end in itself. Indeed, because of this, the greatest fascination of this book is likely to be for those who have been personally connected with "The Brompton." They will surely fall under the spell, and as night falls on the end of an epoch in 1948, may have to shake themselves to remember that the future holds prospects of continued and increasingly valuable service to the sick—now as a right and without charity—as well as to medical knowledge.

ELIZABETH ARMSTRONG. *Robert Estienne, Royal Printer. An Historical Study of the Elder Stephanus.* Cambridge, at the University Press, 1954. xxi, 310 pp. $10.00.

Reviewed by the EDITOR.

Robert Estienne is one of the greatest figures in the history of the art of printing, and Miss Armstrong, who has obviously given close scrutiny to original sources in the libraries of France and England, tells the story of the Estienne family with authority and charm, and the Cambridge Press has put her text and her well-chosen illustrations in a form worthy of the Estiennes.

Estienne lived in troubled times; he published for John Calvin and knew Rabelais, Servetus, and Etienne Dolet and, despite his independence of mind, managed to escape the fiery fate of Servetus and Dolet even though basically he was a rebel against both conventional printing forms and society.

Following a brief but informative introduction, the text is divided into five "books": I, The Merchant-Craftsman, II, The Scholar-Publisher, III, The King's Printer, IV, The King's Printer and the Censors, and V, Geneva. The story of the origin of the Estienne press is one of the most dramatic in the history of printing, and the later development of Estienne's prestige as a printer is one that merits the closest study. As printer to François I and Henry II, he was made a member of the court and moved freely among the most influential people in France. After his removal to Geneva following turbulent times in Paris, he settled down to a more quiet existence although he still, even there, had trouble with the censors.

We are all most grateful to Miss Armstrong for this vivid picture.

J. SEIDE. *History of Medicine.* Tel Aviv, M. Newman, 1954. 205 pp.

Reviewed by JOSHUA O. LEIBOWITZ, *Medical Historian, Jerusalem-Katamon, Israel.*

THE reviewer's task in the present instance is charged with greater responsibility than usual since the volume under consideration is written in Hebrew, a language known to but a small number of the readers of this Journal. The possibility of the reader's making his own judgment is hence rather restricted, a situation often met with also in the case of comment on books written in Scandinavian and Slavonic languages.

The author deserves praise for having written the first history of medicine in Hebrew. The book is well organized and it contains indeed all the facts and personalities which are important enough to be included in a relatively small volume. Furthermore, the author has tried to trace the rôle played by Jewish physicians from antiquity to the present time. This he has succeeded in doing tactfully and without undue exaggeration. To include local achievements in a general presentation is a common practice with smaller nations; the eminent scholar Edv. Gotfredsen, for instance, did likewise in his Danish history of medicine.

The volume under review contains two chapters on Biblical and Talmudic medicine. Here the author remarks rightly that "much more additional research is yet to be done." In former days Biblical medicine attracted more attention, and even well-known clinicians, such as Thomas Bartholin and Richard Mead, contributed to this field. Despite some noteworthy recently published papers a comprehensive presentation, inspired by the methods of modern historical research, is badly needed.

Dr. Seide's work is divided into 17 chapters and contains 44 illustrations. Preceding the section on Greco-Roman medicine, one chapter covers the subject of Indian and Chinese medicine and one the PreColumbian American art of healing. The middle ages, the Renaissance, the modern era are adequately dealt with. A special feature is the inclusion on the Marrano physicians: Garcia da Orta, whose Jewish origin was unknown till the publication by the Portuguese historian Augusto da Silva Carvalho in 1934; Christoval Acosta, Amatus Lusitanus, Montalto, Zacutus Lusitanus, Roderigo de Castro. The dynamic advances of the eighteenth and nineteenth centuries are clearly outlined, and a chapter on "Contemporary Medicine" is appended, where an account of tendencies and newer techniques, including teamwork, replaces the biographical and more colorful presentation of the earlier periods. Then follows a rather comprehensive chapter entitled, "Jewish Physicians in Modern Medicine," and finally one which lists the Jewish Nobel prize winners in medicine; here twelve scholars are chronologically enumerated with short notes on their life histories and achievements.

The author's erudition in many fields of the history of medicine and botany, his broad culture, and lucid, concise style make the book highly recommendable.

JOHN L. THORNTON and R. I. J. TULLY. *Scientific Books, Libraries and Collectors. A Study of Bibliography and the Book Trade in Relation to Science.* London, The Library Association, 1954. x, 288 pp. 12 plates. 24s.

Reviewed by W. B. MCDANIEL, 2D, *Curator of Historical Collections, Library of the College of Physicians of Philadelphia, Philadelphia, Pennsylvania.*

IN 1949, Mr. Thornton, Librarian to St. Bartholomew's Hospital Medical College, published a book entitled *Medical Books, Libraries and Collectors.* The present book, covering, obviously, a very much broader field, follows exactly the organizational pattern of the earlier one. The twelve chapters are headed as follows: Scientific Literature Before the Invention of Printing; Scientific Incunabula; Scientific Books of the Sixteenth Century; Seventeenth-century Scientific Books; Scientific Books from 1700-1799; Scientific Books of the Nineteenth Century; The Rise of Scientific Societies; The Growth of Scientific Periodical Literature; Scientific Bibliographies and Bibliographers; Private Scientific Libraries; Scientific Publishing and Bookselling; Scientific Libraries of To-day. The impressive bibliography occupies 27 pages; the index, 18.

It is important to know what an author had in mind to accomplish. The Messrs. Thornton and Tully are agreeably explicit on this point: "We have not attempted to make this book an exhaustive treatise on the bibliographical aspects of science, but rather an introductory history of the production, distribution and storage of scientific literature from the earliest times. . . . The professional historian of science will find little new in these pages, but the student of the subject, and the scientists searching for 'bibliographical gaps,' will find between two covers an accumulated wealth of material on the bibliography of science."

In straightforward, concise fashion, the authors present a very generous amount of information, based on a wide reading of the most authoritative, though in many cases not easily accessible, sources. As the chapter headings may suggest, one inevitably encounters a number of persons, societies, and publications in more than one place in the text.

Mr. Thornton's earlier book was candidly, but not unkindly, fitted into the "semi-readable" category, with the suggestion that the plethora of information presented was conducive to mental indigestion. The same criticism, naturally, will here and there be made of this companion volume. The book will doubtless become a required manual in training courses for science—and, one might hope—general librarianship. It will serve many as a useful reference work. Since "readability" depends in large measure on

the reader's interest in a book's contents, and this one covers a field so broad that relatively few could have written it out of their heads, there may well also be students of segments of science interested enough to read, and quite profitably, straight through the book. The student, in his armchair, is, after all, under no compulsion to absorb for permanent instantaneous use every fact presented to him. And for indigestion caused by overeating, there is an old fashioned preventive remedy that may safely be self-administered. It is not to stop eating entirely, but to eat in moderation.

Basler Veröffentlichungen zur Geschichte der Medizin und der Biologie, herausgegeben von PD. Dr. med. H. Buess. Basel, Benno Schwabe & Co. Verlag. Fasc. I. Beate Moeschlin-Krieg: *Zur Geschichte der Regenerationsforschung im 18. Jahrhundert,* 1953, Fasc. II. Hans Rudolf Thür: *Johann Balthasar Luchsinger (1849-1886). Ein wenig bekannter schweizer Physiologe,* 1953, 8 Fr.; Fasc. III. Max Winzenreid: *Das Militarsanitätswesen in der Schweiz von der Mediation bis zum Sonderbundskrieg,* 1954, 11 Fr.; Fasc. IV. Ernst Rolf Buser: *Zer Entwicklung des Badewesens im Unterengadin. Ein Beitrag zur Geschichte der Balneologie der Schweiz,* 1954, 5.50 Fr.

Reviewed by GEORGE ROSEN, *Professor and Head of the Department of Public Health Education, Columbia University, New York City.*

THE publication of works of scholarship is beset by many problems, but the greatest difficulty is generally encountered in trying to publish studies that are not long enough to appear as books nor short enough for a journal. A solution which helps to solve the problem is the creation of an independent series of monographs, frequently issued in soft covers, and under the sponsorship of a department in an institution of higher learning. Examples in the field of medical history are the *Abhandlungen zur Geschichte der Medizin und der Naturwissenschaften,* initiated by Diepgen at Berlin in 1934, the *Supplements to the Bulletin of the History of Medicine* started by Henry Sigerist at Baltimore in 1943, or the *Collana di studi di storia della medicina* which began to appear at Milan in 1945. Many others could be cited.

It is a pleasure to welcome a new series of this kind initiated in 1953 at Basel under the editorship of Dr. Buess, who is already well known to medical historians. The first four volumes of the series are now before us. They deal, in order of appearance, with the history of regeneration research in the eighteenth century, a forgotten Swiss physiologist of the nineteenth century, military medicine in Switzerland from the beginning of the nineteenth century to the Swiss War of Secession (1847), and the development of the medicinal baths and spas in Unterengadin. As might be expected, these monographs exhibit a strong emphasis on Swiss medical history. Switzerland has played a significant rôle in the development of medicine so that such studies are of more than local interest.

It is not possible to offer a detailed analysis of each of these four monographs. Dr. Moeschlin-Krieg's study of regeneration research in the eighteenth century is probably of the broadest general interest. The author deals in turn with Réaumur, Trembley, Bonnet, Rösel von Rosenhof, Schäffer, Müller, Spallanzani, and others less well known. After discussing the investigators and their experiments, the contending theoretical interpretations of the results are considered. Analyzing the evolutionist and epigenetic theories and their proponents, the author points out that these apparently irreconcilable positions have today been synthesized by modern biology.

The study of Johann Balthasar Luchsinger, a Swiss physiologist, who died at the early age of thirty-seven, is of interest not only as a contribution to the history of physiology in Switzerland. Luchsinger studied with Carl Ludwig, but he was also very strongly influenced by the work and example of Claude Bernard. As a result he combined the methodology of the French and German schools and thus helped to lay the experimental foundation of modern physiology in Central Europe.

The monographs by Winzenried and Buser are essentially of local interest. However, anyone concerned with the history of military medicine or of balneology will probably find the material contained in them of some interest.

The publication of these four monographs is indeed an auspicious beginning and leads one to look forward with anticipation to future publications in this series.

A. H. Stanton and M. S. Schwartz. *The Mental Hospital.* The Basic Books, Inc., 1954. 492 pp. $7.50.

Reviewed by Stephen Fleck, *Physician-in-Charge, Yale University Psychiatric Institute, New Haven, Connecticut.*

It has been 110 years since thirteen mental hospital administrators met to found a national organization—the precursor of the American Psychiatric Association—and only now has a book-length scientific study of life inside a psychiatric institution appeared. A series of subjective autobiographical accounts of patients' hospital experiences has been with us for a century or more. Beginning with an anonymous account (Introduction to the *Philosophy of Insanity* by F. Fromm-Reichmann), this type of publication reached high points of impact on the public with Clifford Beers' *A Mind That Found Itself* and, in our times, with Jane Ward's *Snake Pit.* Such writings, like those of Dorothea Dix in the last century, have kept the mental hospital problem before the public eye, have helped sporadically with appropriations, and have led to significant improvements, primarily in those areas where the institutional and the outside communities interact. They apparently influenced least the physicians most intimately concerned with patient care.

Now Doctors Stanton and Schwartz present us with an objective account of hospital life, based upon participant and non-participant observations, which no hospital psychiatrist can afford to ignore. It is the combination of both methods which renders their book as valuable as it is original. It makes comfortable reading only for those hospital psychiatrists and administrators whose primary aim is to have "well-behaved" patients and "a peaceful institution."

For such, if they exist, it will be particularly easy to advance as criticism that a small private profit-making institution is not a typical sample of mental hospitals, as indeed it is not. The hospital under scrutiny is one of the pioneer institutions in the field of psychotherapy with psychotic patients and is largely staffed by analysts or analysts-in-training. Atypical as the hospital may be, Stanton and Schwartz document convincingly that because of the discomforting nature of such a study, it could only be carried out in a setting where psychological sophistication pervades the entire team.

It is true that the description of process and interaction on the one ward used for the study are not readily observable in a different setting with different patients, and lend themselves even less readily to the formulation of therapeutic and management solutions in different institutions. One of the values of this book lies in the demonstration that participant studies and analyses can be carried out in such a setting, that the data can be validated, and that both can be done without undue disruption of service. This book, therefore, has very general value. It highlights how patients spend "the other 23 hours" (other than the one given to psychotherapy). There are searching discussions of methods and practices common to all hospitals. Many of these are traced to personnel needs, exposing almost universally recurring rationalizations and blind spots, such as why psychotherapists blandly overlook ward realities or, in reverse, the use of "rules" to avoid meeting special needs of patients which should and could be fulfiled.

The book is not a mental hospital encyclopedia. It is limited to those processes actually studied, but one may hope that the authors will continue their work by focussing on other integral problems such as interaction between hospital and the patients' families, or the relationships between treatment and custodial sections, to name but two.

The rôles of all team members as well as the "rôle of being a patient" are described and analyzed, and the authors' deductions appear sound and pertinent far beyond the specific happenstances recited. Collective ward disturbances come in for close scrutiny; the authors leave the impression that they are generally related to staff disagreements, which may be a somewhat sweeping deduction. There is better evidence to support the formulation that individual excitement is related to interference in effective communication which either entails or is based, sooner or later, on misunderstanding among the staff.

The authors are to be congratulated on a fundamental piece of work, the most important aspect of which is their pointing up and elucidating the "participant rôle of the psychiatric hospital patient." They have brilliantly fulfiled the prediction of Samuel B. Woodward (the chosen President of the thirteen founders) that "what is overlooked by the passing generation, which might have been beneficial (to patients), will be supplied by their successors."

Stanton and Schwartz tell us a great deal that has been overlooked by a good many generations.

NATIONAL HISTORICAL PUBLICATIONS COMMISSION. *A National Program for the Publication of Historical Documents: a Report to the President by the National Historical Publications Commission.* Washington, U. S. Government Printing Office, 1954. viii, 106 pp. $.50.

Reviewed by JOHN B. BLAKE, *Assistant Historian, The Rockefeller Institute for Medical Research, New York City.*

Of concern to everyone interested in any phase of American history is this report of the National Historical Publications Commission. To make more generally available documentary material, chiefly manuscript, relating to the nation's development, the Commission recommends the compilation of guides to depositories and the publication of the papers of a number of influential figures of our past. An appendix presents a preliminary list of 112 persons recommended as worth including in the program, with a brief summary of the career of each and some indication of the present status of his papers. Believing that "these documents should be selected for the purpose of providing information not only about our political and military history but also about our economic, social, and intellectual development," the Commission has included individuals of diverse attainments. Among them are a generous sprinkling of scientists, philanthropists, social workers, educators, and reformers of at least tangential interest to medical historians. Directly representing the medical profession are John Shaw Billings, Oliver Wendell Holmes, Joseph Leidy, William and Charles Mayo, and William H. Welch; another five physicians are included in a supplementary list of 249 additional suggested names. In view of the Commission's broad outlook, readers of this *Journal* may well endorse its efforts to make the records of leaders in many fields more readily available and therefore more useful to scholars everywhere.

NOTES ON CONTRIBUTORS

JESSIE DOBSON, B.A., M.Sc., is Recorder, Royal College of Surgeons, Lincoln's Inn Fields, London W.C. 2, England.

W. W. FRANCIS, M.D., is Curator of the Osler Library, McGill University, Montreal, Canada.

JOHN GOLDMAN, who collects coins of medical interest, is a student of Walnut Hills High School, Cincinnati. He is the son of Dr. Leon Goldman, who is Professor of Dermatology at the College of Medicine of the University of Cincinnati.

HEBBEL EDWARD HOFF, M.D., D.Phil., is Professor and Chairman of the Department of Physiology, Baylor University, Houston, Texas.

JOHN FELIX MARCHAND, M.D., is on the staff of the Department of Medicine, Cornell Medical College, New York, New York.

CHARLES DONALD O'MALLEY, Ph.D., is Professor of History, Stanford University, Stanford University, California.

GEORGE SARTON, Ph.D., is Professor Emeritus of the History of Science, Harvard University.

THEODORE L. SOURKES, Ph.D., is Lecturer in Psychiatry, Allen Memorial Institute of Psychiatry, Montreal, Canada.

LLOYD G. STEVENSON, M.D., Ph.D., is Honorary Librarian of the McGill Medical Library, McGill University, Montreal, Canada.

LYNN THORNDIKE, Ph.D., is Professor Emeritus of History, Columbia University, New York, New York.

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